

## CONTENTS

Editorial Note . . . . .	11
SOME PROBLEMS FOR MEDITERRANEAN ANTHROPOLOGY . . . . .	81
Roger Just, Institute of Social Anthropology, Oxford.	
PRATTLE AND POLITICS: THE POSITION OF WOMEN IN DOSHMAN-ZIARI . . . . .	98
Sue Wright, Department of Ethnology and Prehistory, Oxford.	
TALKING ABOUT SHAMANS . . . . .	113
Roger Rouse, Institute of Social Anthropology, Oxford.	
GRASSHOPPERS AND SLUGS . . . . .	129
A Comment on Ovesen, Chapman and McDonald	
Vernon Reynolds, Department of Biological Anthropology, Oxford.	
THE MISSING LINK . . . . .	131
A Reply to Reynolds	
Malcolm Chapman, Institute of Social Anthropology, Oxford and Maryon McDonald, Wolfson College, Oxford.	
GERTRUDE: A POSTFACE TO A FEW PREFACES . . . . .	133
REVIEW ARTICLE: THE DUTCH CONNECTION . . . . .	143
Anthony Shelton, Institute of Social Anthropology, Oxford.	
BOOK REVIEWS:	
Aziz, B.N.: <u>Tibetan Frontier Families</u> - by Graham Clarke . . . . .	149
Spengler, J.J. (Ed.): <u>Population Problems in the Victorian Age</u> - by Phil Kreager . . . . .	151
Gutman, H.G.: <u>Work, Culture and Society in Industrializing America</u> - by William Cronon . . . . .	153
BOOKS RECEIVED . . . . .	156



(ii).

#### FORMAT

The journal is published three times per year. Articles are welcome from students of anthropology and from people in other disciplines. Papers should be as short as is necessary to get the point over. As a general rule they should not exceed 5,000 words. We welcome comments and replies to published articles. All papers should follow the conventions for citations, notes and references used in the A.S.A. monographs. Communications should be addressed to the Journal Editors, Institute of Social Anthropology, 51 Banbury Road, Oxford.

#### SUBSCRIPTIONS

The cover charge for a single issue is 40 pence; the subscription rate is - single issues: 70 pence or \$2 for individuals, £1.20 or \$3 for institutions; for one year: £2.00 or \$4 for individuals, £3.50 or \$8 for institutions. All prices cover postage. Vol II no. 1, Vol III no 2, Vol. IV, Vol V no. 1, Vol VI, no. 1 and Vol. VII no. 3 are no longer in print. Other back issues are available at the prices quoted above. Cheques should be made out to J.A.S.O. and sent to the Journal Editors, 51 Banbury Road, Oxford, England.

Thanks are due to all those who have assisted with the production of this issue, particularly Sue Bright and Teresa Dragadze.

#### NOTE

Gertrude: A Postface to a Few Prefaces first appeared in Cahiers d'Études Africaines, XVII:1, 1977, pp 177-187. The translation used here is by Maryon McDonald.

## SOME PROBLEMS FOR MEDITERRANEAN ANTHROPOLOGY

### I.

This paper has a less than admirable provisionality, for in it I want to air some of my misgivings about the nature and direction of the new and burgeoning field of Mediterranean anthropology, and while my misgivings arise from two sources -- a reading of published ethnographic material, and my own limited field-work in an Ionian island village -- certainly I could not claim to have read the entire corpus of Mediterranean studies, and thus far I have conducted only six months field-work in the village (during which time my grasp on the language was tenuous to say the least). Any general criticism I make of Greek, Mediterranean, or even European anthropology must, therefore, be seen as precocious and, in the strict sense of the word, ill-founded. In fact I would be only too happy to have my assertions rejected and my misconstructions pointed out. That is the hope in which this paper has been written. But secondly, though my specific concern is with certain problems which I think to be inherent in the anthropology of Mediterranean (or European) societies, unfortunately they are not, in my opinion, new problems peculiar to this ethnographic sub-division of the discipline; rather, I think it the case that Mediterranean or European anthropology merely throws into new and peculiar relief certain very old problems which lie at the base of the anthropological enterprise as a whole. This being so, I find myself taking on the world -- at which point I become a little confused. I hope, therefore, that I may be given leave to proceed in a somewhat erratic fashion.

### II.

Let me start with a digression. Anthropology, rather like philosophy, is not amenable to neat and easy definition. It is often quite embarrassing to be asked point-blank by an earnest layman, 'What is social anthropology?' For anthropology has failed to accumulate over the years any tried and trusted body of explanatory theories, strategies and methods which could be applied confidently to whatever field of study the anthropologist has at hand. Rather, we now find ourselves the heirs to a debris of competing -isms, occasional insights, ad hoc speculations, and dangerous generalizations from which we select as necessity rules. In short, anthropology cannot be defined in terms of a distinctive intellectual practice (though personally I find that no bad thing). More to the point, anthropology can no longer be defined even in terms of a distinctive subject matter -- at least not in any way which would clearly establish for it its own and exclusive domain free from the incursions and prior claims of the 'cognate disciplines'. If, in the light of its present diversity, we are reduced to saying that anthropology is somehow concerned with 'the social', veracity is gained at the expense of integrity.

In the past this was not so. Anthropologists could state with a fair degree of confidence that however they did it, they did know what it was they studied: primitive societies. Of course Evans-Pritchard (and a good number of others) was very wary of the 'primitive' even by 1950, for it did not mean 'that the societies it qualifies are either earlier in time or inferior to

other kinds of societies' (1951:7). Nevertheless, though the word was 'perhaps an unfortunate choice . . . it has now become too widely accepted as a technical term to be avoided' (ibid). And as a 'technical term' it referred to 'those societies which are small in scale with regard to numbers, territory, and range of social contacts, and which have by comparison with more advanced societies a simple technology and economy and little specialization of social function' (ibid:8) -- to which might be added the further criteria of 'the absence of literature, and hence of any systematic art, science, or theology' (ibid). It should be stressed that Evans-Pritchard was accurately reporting the state of play, and that he did foresee changes to come. Studies of 'non-primitive' societies had begun already,<sup>2</sup> and he emphasizes that,

theoretically at any rate, social anthropology is the study of all human societies and not merely of primitive societies, even if in practice, and for convenience, at the present time its attention is mostly given to the institutions of the simpler peoples, for it is evident that there can be no separate discipline which restricts itself to these societies (ibid: 10, my emphasis).

But 'primitive societies' were nevertheless anthropology's 'convenient practice', and their study was de facto its definition.

The state of play has, however, changed. Now one finds it neither very practical nor very convenient to study 'primitive societies' -- and let me momentarily forestall problems of definition simply by saying that it is increasingly difficult for anthropologists to work in Africa, South America, Melanesia, and other areas of traditional interest. But whereas Evans-Pritchard could state that there was no theoretical necessity for anthropology to be limited to the study of 'primitive societies', as anthropological practice has in fact changed, it is now held as an article of some definitional importance by those in the fore-front of the field that anthropology is certainly not the study of 'primitive societies'. Thus Jan Ovesen in a recent review article (1978:1) can refer to the 'traditional but erroneous opinion that anthropology is the study of primitive societies', and Beidleman can severely take to task I.M. Lewis' publication last year of an anthropological primer on the grounds that, 'The emphasis is overwhelmingly upon exotic, preliterate societies (far too many from East Africa) distant from most readers' experience', whereas, 'Some of the most provocative and far-reaching work in anthropology during recent decades has been as much concerned with research in Western societies and cultures as with others' (1977: 741).

Now the present diversity of anthropology and the present flight from the 'primitive' as the substantive marker of the discipline have to do with much more than mere shifts in ethnographic location. A variety of theoretical stances and concerns has also led us away from the (seemingly) straight-forward description and documentation of the 'simpler peoples'; indeed one could argue that a definition of anthropology as the study of 'institutions' is as passé as a definition of it as the study of 'primitive societies'. Nevertheless, to take an example from current rhetoric, the from 'function' to 'meaning', from 'institution' to 'communication' is, I think, intimately connected with a shift in the empirical base of anthropology from the study of 'primitive peoples' to the study of, let us say, 'the forms of human experience'. Suffice to note that the passing away of the conception of anthropology as a strictly empirical discipline removes the necessity of locating for it a strictly defined empirical field of

study, i.e. 'primitive societies'. But there are other reasons for the demise of the 'primitive' which, with the advantage of hindsight, appear within the history of anthropology to have an almost logical inevitability.

In the nineteenth century the idea of the primitive may not have been well-defined, but it was well understood; that is to say its referents were unambiguously communicated. Primitive societies were both chronologically prior and inferior, and one knew where to find them, which peoples they were. And given that anthropology operated under the cover of a generally accepted evolutionary theory, it was equipped with both a clear object of study, and a clear reason for studying it. There were ourselves and others, and the distinction was qualitative: civilization and savagery. The interest lay in hypothesizing the transition. But with the advent of field-work the great change commences. Savages (and their sex lives) continue to exist; so do primitives (with their law and economics); but historicist speculation is banished, and with this a certain integrity is granted the 'simpler peoples'. At least they are no longer malingerers on the road to civilization. They are fully-fledged societies in their own right. But, once the teleological relationship between savage and civilization is thereby broken, and once, furthermore, a considerable effort is expended in showing the 'rationality' of bizarre beliefs and customs, the 'internal cohesion' of odd ideas and practices, then obviously it becomes increasingly difficult to maintain any clear-cut qualitative set of differences upon which to base a radical division of the world into 'them' and 'us', into 'primitives' and 'non-primitives'. Hence the inverted commas; hence the 'technical definition' -- except, of course, that it was anything but a technical definition. It was extraordinarily pragmatic and ad hoc definition straining merely to encompass by the enumeration of certain rather arbitrarily selected features, and none too successfully at that, those societies which anthropology had established already on other grounds to be the objects of its study. In fact, if anthropology was not studying primitive societies (without the inverted commas), and if there was no special hierarchical relationship between primitive societies and non-primitive societies, then anthropology was merely studying 'other societies', and the choice of which other societies was an historical contingency. Anthropology becomes a sociology of the exotic, and what is exotic becomes strictly relative to the culture of the anthropologist.

This being so, without any need to consider the practical difficulties now attendant on pursuing studies in the traditional stamping grounds of anthropology, it becomes almost the result of the progression of anthropology's own internal logic that the question should be put, 'Why not study our own culture?' or 'Why not study those European cultures whose traditions overlap our own?' After all, if one cannot define the primitive, and if the primitive has no special place in the grand scheme of things, and if, consequently, our own society has no privileged status, and if we are all relativists now, then to continue to limit our studies to a handful of societies whose only criteria of selection lie in a discarded theory of the past seems simply perverse. One sees Mary Douglas arguing almost exactly this. The most Durkheimian of present anthropologists is compelled to take Durkheim to task because he did not 'push his thoughts on the social determination of knowledge to their full and radical conclusion' (1975:xi), blocked as he was by two unquestioned assumptions: that primitives were utterly different from us, and that part of our own knowledge was grounded in objective scientific truth. And to make amends, Mary Douglas attempts an across-the-board analysis of cultural symbolism and categorization in

which the Lele pangolin and the European meal are deciphered between the covers of one book. It is a noble enterprise with which I would not quarrel, and the logic of its undertaking is compelling; but though I would not wish to reassert the notion of the 'primitive', certain problems are generated by such an approach and certain other considerations obscured.

Let us accept the relativity of knowledge; let us dispense with any notion of the primitive, any privileged status for our own society; let us assume that in theory European societies are as susceptible to anthropological scrutiny as any others. One salient feature of traditional anthropology, perhaps its characteristic feature, remains unaffected by any reconsideration of the status of the primitive vis à vis the civilized: namely, that those societies with which anthropologists traditionally have concerned themselves were at least different from our own. Neither more nor less complex than our own, perhaps; nor different as a class of societies from our own, perhaps; but nevertheless each different in its own way from our own. The point is not insignificant, for it substantially modifies any view (a view which I have been trying to trace) which sees the dissolution of any categorical barrier between 'them' and 'us' in terms of 'primitive' and 'non-primitive' as rearranging the societies of the world in such a way as to make the decision to work in Spain or Greece or England or Ireland quite as viable a choice as to work in the Sudan, or the Amazon or New Guinea. In retrospect and with all the advantages of hindsight I would assert that it was never the question of the societies which anthropologists studied being simpler or more primitive than our own which was important; it was the fact -- the very simple fact -- that they were just different from our own which gave anthropology its particular perspective and allowed its particular insights.

The odd thing is that this is well recognized, and that its recognition stems from almost the same train of relativist reasoning which we have been attempting to trace. It is recognized in Edwin Ardener's language when he talks of the 'critical lack of fit of (at least) two entire world-views, one to another' -- a critical lack of fit, moreover, from whose apprehension 'the anthropological "experience" derives' (1971:xvii). Indeed, one of the now more oft quoted descriptions of anthropology is in terms of 'cultural translation' -- a phrase coined, I gather, by Evans-Pritchard, but the title of a recent volume of essays edited by Beidelman; Beidelman who latterly has objected because the ethnographic content of Lewis' book was 'distant from most readers' experience'. There is a contradiction lurking here somewhere, even perhaps a paradox; for a while it is a relativist view which in denying the existence of two radically different classes of society holds instead that there are merely 'other societies' all of them equally amenable to anthropological analysis, including our own, it is this same relativist view which in denying any privileged 'scientific' status for our own cultural assumptions reduces anthropology to 'the translation of culture' -- an enterprise which must at least assume that there is something which requires translation into something else, and hence demands (I would have thought) that what is studied be significantly 'distant from most readers' experience.' Indeed one could (unkindly) liken Mary Douglas to someone who, having methodically moved along the surface of a Moebius strip, did not realize that suddenly she was facing the other way round; for whilst in the absence of any categorical distinction between 'primitives' and ourselves the move from studying exotic cultures to studying our own seems a logical and innocuous progression, precisely because we are dealing with mere relative

points of view the enterprise in fact has changed radically: in the first case the task was to encounter the seemingly strange and bizarre and, by a process of 'cultural translation', to render it acceptable to 'common-sense' so that we might understand it; in the second case the task is to take the accepted and common-sensical and, by some other process of 'translation', to render it momentarily strange and bizarre so that we might seek to understand it.

Now I am not arguing that either of these tasks is invalid, or that as anthropologists we should not pursue them both. The point is that they are not tasks of the same order, and that for us the study of our own society and those basically similar to it does not form a continuum with the study of exotic, 'different' societies. To assume it does is misleading. True, one can argue that the study of exotic societies has always led to a reconsideration of the notions, categories, concepts and even institutions of our own society; nevertheless, a reflection on the validity, or the arbitrariness, of one's own cultural assumptions brought about by their confrontation with those of another society is not at all the same thing as an attempt to see beneath the surface of one's own culture by means purely of a rigorous scepticism or a sort of self-induced alienation. It was, after all, always the practical problem of cultural translation, the 'critical lack of fit' between one's own culture and another, that supplied the empirical basis for investigation.

The first thing to be said is that the investigation of one's own cultural apparatus is a very much more difficult affair. As Needham has stated,

. . . we too must be thinking about social facts in comparably invalidating ways, and [that] in our case also the influences responsible can in principle (if not now) be identified . . . Obviously, this radical kind of critique is the hardest of intellectual undertakings, for we cannot by deliberation alone detach ourselves sufficiently from those tacit premisses which themselves frame or constitute thoughts -- but we have to try (1976:84).

Yet those 'tacit premisses' are inextricable, presumably, from what Mary Douglas refers to as 'implicit meanings' and which she would have us uncover and explore in the context of our own society as much as in the societies of others.

But there is more than a question of relative difficulty involved -- at least if we are concerned with the writing of ethnography. It is hard, of course, to say what the writing of ethnography should aim now to be, but one would imagine that a still indispensable requirement was that something of the experiential nature of the society investigated should be conveyed. The old question was whether this ever really could be achieved. Could we come to think of witchcraft, or think with witchcraft, in the manner of a Zande? Perhaps not. And translation remains translation -- necessarily a distortion. But given alien institutions and alien concepts, a form of translation was the best that could be hoped for; it constituted our only means of grasping the unfamiliar. But what happens when we strike the already familiar and proceed to render it into something else, to treat it in the same way as we have been accustomed to treat the exotic? The net effect is that something which was in the first place quite 'comprehensible', which in



no way offended common-sense, is made to seem mightily peculiar. This is all right if we know what we are up to. If Mary Douglas attempts a structural analysis of a meal, we are not given to suppose that the form of her analysis corresponds to what that meal means for us, or to the way we see it as the almost daily participants in that little piece of ritual. She is uncovering implicit, not ostensible, meanings. But if, as anthropologists returned from the field, we report on the habits of the Greeks, or the Italians, or the Spaniards, habits which, for the most part, I would assert not to be truly bewildering and not to create any crisis of 'understanding' in the generally accepted sense of the word, and if we report on them in the manner, say, of Mary Douglas, then we run the risk of making the peoples who inhabit our ethnographies seem very much more strange and alien than in fact they are. We run the risk of 'anthropologising' them.

Now what I have attempted to do so far is to sketch in the background to what I consider to be a fundamental problem for Mediterranean anthropology. That it should constitute a fundamental problem rests, of course, on my assertion that in the Mediterranean we are dealing with societies basically similar to our own, or at least with societies which, on the surface of it, are comprehensible in terms of our own 'common-sense' assumptions about the nature of the world. To justify this assertion empirically would be impossible. Intuition has to remain the ultimate arbiter of what does and what does not require 'cultural translation', just as in the study of language itself an appeal to linguistic intuition has to be made to determine what does and what does not fall within a particular language's bounds. But if we turn to some of the anthropological writings on Mediterranean societies, I think we can see that the problem does exist, and that there are a range of difficulties which it creates.

### III.

John Davis' People of the Mediterranean (1977) is a convenient point of departure, since it offers a review of most work up to 1975. In as much as Davis' book is polemical, it has two contentions: that Mediterraneanists have failed to be comparativist, and that they have failed to be historical. The actual validity of these contentions need not concern us here; what is worth considering are the implications of Davis' proposed remedies; for at almost every stage what Davis desires is the collection of a body of 'hard', quantifiable, statistical data: income-distributions, land-holdings, migration patterns etc., and though in my own case I am appalled by the difficulties I will face in trying to obtain the sort of information Davis wants, nevertheless I have considerable sympathy with his aims. Anton Blok's The Mafia of a Sicilian Village, 1860-1960 (1974) receives high praise from Davis for its thorough investigation of the economic conditions which have obtained in 'Genuardo', backed up, as it is, by the material of local records, agricultural reports, and actual numerical data. I would consider Jose Cutileiro's A Portuguese Rural Society (1971) to be a similarly praise-worthy book, and Loizos' writings fall into the same class.<sup>3</sup> But -- and here is the crunch -- admirable though these works are, if they are to be the models of future research, what they spell is that the anthropologist will have simply to join the ranks of the social and economic historians. Whatever it was that really distinguished the great ethnographies of the past, which distinguished anthropology as a subject from history and sociology, has gone; gone because what is lacking is precisely that 'critical lack of fit' between 'two world



views' which prompted the investigation of a range of alien concepts and categories and which, conversely, led us to reflect on the nature of our own habits of thought. What replaces this is the steady compilation of empirical data which, rather than challenging our own world view, merely documents the unknown course of those entities which already inhabit our conceptual universe.

For many (and I suppose I would include myself amongst them) this does not seem very satisfying. We are not attracted to anthropology by the prospect of a life spent counting sheep -- even supposing that there are very many sheep left to count. The need (and perhaps it is merely a romantic need) to assert the distinctive nature of anthropology and its contribution to the study of human society is still there. And here we note something of a split in Mediterranean anthropology. I stress, however, that I am not talking about two camps or schools within the subject; rather, every now and again it seems that anthropologists of Mediterranean societies do want to show that they are more than local historians or village sociologists.

Two books stand out as exemplars of 'traditional' anthropological concern within the Greek context: the justifiably renowned works of John Campbell and Juliet du Boulay, respectively Honour, Family and Patronage: a study of institutions and moral values in a Greek mountain Community (1964), and Portrait of a Greek Mountain Village (1974). These were, I think, the inspiration of many of us presently trying to work in Greece. But it should be noted that Campbell did his field-work amongst the Sarakatsani in 1954 and 1955, over twenty years ago, and that, moreover, the Sarakatsani were always rather an odd bunch -- transhumant shepherds living a precarious existence in the interstices of settled communities. Du Boulay, for her part, is commendably honest in stating that she was documenting a 'dying village' and a dying way of life. Both, perhaps, represent the last cases of anthropologists being able to find in Greece communities which were significantly alien and significantly at odds with our own world view. The trouble comes in trying to emulate them in other contexts.

In a recent paper (1978), Malcolm Chapman drew attention to two passages from du Boulay's book in which, talking of the imminent demise of her village and its absorption into the larger society of modern Greece, she refers to a

... change from traditional and symbolic thinking to modern and secular thinking (1974:6)

and then later comments that,

... whatever may have been its limitations and its defects, there is no doubt that when it was integrated to a living tradition it gave to life both dignity and meaning -- qualities which are conspicuously lacking in the type of society which threatens to succeed it.

(ibid: 258)

Chapman interprets this as both the cri du coeur of someone who believes herself to have witnessed an historical tragedy, and as an example of a confusion of thought which seeks to oppose the symbolic to the literal and

the sacred to the profane. Yet while I accept Chapman's criticism, I think I can also appreciate the seriousness of what du Boulay has registered; not in terms of an historical tragedy or the triumph of the secular, but in terms of the dissolution of the sort of community sufficiently closed and integrated, and sufficiently different from our own, to allow the writing of traditional holistic ethnography with all its concern for the inter-relatedness of things and an alternative view of the world; for now that such communities are passing away, regretfully I find myself on the side of the sheep-counters. To do otherwise seems peculiarly dangerous.

In his Curl Prize essay (1969), Davis commences by stating that,

Honour has now the status of an ordering concept used by anthropologists of the Mediterranean basin to bring together a variety of phenomena which are not found in all Mediterranean communities and which are not always related in quite the same way in different societies. (1969:69)

As Davis makes clear, 'honour' is not without its problems when used cross-culturally since it tends to group rather disparate phenomena. But even when one sticks to a single community, the use of 'honour' as an 'ordering concept' is not without its difficulties. Despite its prevalence in writings concerned with modern Greece, the odd thing is that the indigenous terms which it glosses -- timé (honour, worth, value), philotimo (honour, pride, dignity, self-esteem), egoismos (pride, self-regard) and their reflexes, and dropi (shame, modesty, prudence) which is generally included in the same complex -- are not words which one finds regularly or even frequently on everybody's lips. Whatever 'honour' is within the Greek context, it is not the translation of a single high-frequency indigenous term. Rather it seems to be used by anthropologists to embrace a variety of verbal statements (sometimes as simple as the statement that someone or something is good or bad), and to embrace a wide variety of non-verbal, or not specifically verbal, behaviour which is seen by the anthropologist (and hopefully by the Greeks themselves) to relate to the concept of honour.

Well and good; I am not arguing that anthropologists should restrict their analyses of the operations of a particular concept to the investigation of the use of a particular word -- though it must be admitted that this situation does not allow a rather free interpretative hand. But since 'honour' is not an entity, real or imagined, and must be understood rather as a notion which both motivates an individual's actions and is used by an individual to classify and judge other people's actions (or situation), one does tend to find anthropologists leaping inside people's heads with alarming alacrity. Again, I would not want to dispute the welter of evidence (and indeed my own observations) that Greek moral judgements are often rather different from those, say, of an Englishman, and that the concept of honour does constitute an important part of that difference. In the hands of so skilled an observer and writer as du Boulay, for example, the concept of honour does emerge fully-fleshed from the mass of intricate detail which it resides. But if 'honour' is a 'collective representation' of singular importance within the Greek context, it must also be admitted that it is a collective representation of a particular and rather difficult sort. For it does not resemble even so intangible an entity as, say, 'soul' about which people can express at least certain ideas as to its nature, location,

ontological status etc., and this regardless of whether or not they actually 'believe' in its existence. 'Honour', on the other hand, is expressed and formed in the context of mutual assessments and estimations, and in the absence of the regular usage of a particular term, it is surely too easy for the anthropologist also to enter into a series of classifications and judgements which are then placed under the rubric of 'honour' and attributed to the people being studied. After all, if I am offended by someone's actions, a slight or an insult (as occasionally I am), and I show my offence in some way or other, it would not be difficult to credit me with a notion of 'honour' and to say that it was my 'honour' which had been offended -- especially if my use or non-use of the term 'honour' was deemed unimportant. Without wishing, then, to discredit the work of the better Mediterraneanists, I do think it the case that we have been forced to enter a field of investigation which is surely amongst the most difficult and uncertain: namely, the notions and feelings that inform and motivate individuals' actions. And in the hands of the less subtle, the ascription of 'honour' to the Greeks as a primary value the possession of which makes them immediately from ourselves, starts, I think, to come very close to ethnographic distortion.

But I have just used the word 'forced', and I think this is the crux of the matter; for in the northern Mediterranean context the alternative to 'sheep-counting' so often appears to entail just such dangerous fields of investigation. It is, surely, much easier to write about those societies which show gross difference at the 'institutional' level, for example those which practise prescriptive alliance or potlach -- even if, as we know, no clear distinction can be made between 'institutions' and 'ideas'. It is easier even to recover a cosmology than to document an attitude. And it is, I think, the lack of gross institutional differences between our society and Mediterranean societies which forces us to concentrate on other issues generally too *labile* to fix in the language of sociological discourse, without, that is, committing an ethnographic injustice whereby people become as crude as the concepts we employ to describe them.

Family and kinship is an interesting case in point. Somewhat peevishly Davis remarks that it is about time anthropologists started saying something a little more than that in Mediterranean societies the family is 'important' (1977:167). The trouble is that it is quite difficult to say more than that; for the family in Mediterranean societies is rather like our own except somehow more so. On the structural or formal level -- the level which, after all has most exercised the talents of traditional anthropologists -- the Greek, Italian or Spanish family is virtually identical with our own.<sup>4</sup> Variations certainly exist from one community to another with respect to such matters as residence on marriage (patrilocal, matrilocal, neolocal) and the forms of inheritance and dowry, and these are worth investigation; but when one comes really to explain the importance of the family in Greek or Mediterranean societies one finds that it is not a system of rules or any formal qualities which attract notice; rather it is the family's dominance as a moral institution, its closeness, the degree of loyalty it demands from its members, the amount of emotional commitment invested in it, more simply the ubiquity of its mention in daily discourse, which is striking. But once again one is on dangerous ground when analysis is attempted: far away from structure and right into sentiment. Once again generalizations are suspect, because sentiments are malleable. If someone is rich, or powerful, or successful or simply liked, and he is a relative, then the idiom of kinship and the family tends to clothe all reference to him; if he has none of these qualities, or their antitheses,

then the question of his relatedness is seldom mentioned. Family and kinship do not determine sentiments, at least not with any precision, but simply afford the framework for their existence and for their expression, and when one hears it baldly asserted as a matter of established fact that the fundamental loyalties of Greek villagers are to their nuclear families, and one also knows that certain brothers hate each other, that certain sons do not speak to their fathers, and that some mothers bemoan their children (as they do in our own society), then, despite the fact that one knows that being the member of a Greek family is not quite the same thing as being the member of an English family, once again one begins to wonder about the validity of such statements which seek to account for the collective persuasions of a community, and about their validity as generalizations which say something precise about the Greeks which could not also be said about us.

It is, I think, the lack of truly alien institutions, rules, and systems on the one hand, and the sentimental pragmatism which, in their absence, appears to govern actions on the other hand, which has led many Mediterraneanists to espouse something called 'transactionalism' as a label for their activities. It would be out of place for me to argue here what is wrong with transactionalism, but it is perhaps worth quoting a passage from a recent article by Friedl, one of the leading Mediterraneanists, to indicate its direction:

In this situation it seemed best to use the individual as the unit of study. The research strategy assumes that individual actors allocate their time and energy with reference to a set of goals, that they have an awareness of the restrictions of their environmental setting which makes some behaviour intended to accomplish the end rewarding and some not, and that allocations will be continued or changed depending on the actual outcome of earlier behaviour. In other words individuals keep on making decisions, and one important part of the process is the conscious weighing of alternative choices (1976:364).

I dare say, though I don't think this gets us very far,<sup>5</sup> But in an increasingly complex, increasingly mobile, increasingly open society, which more and more resembles our own and less and less expresses an easily discernible coherence, a fixed pattern of institutions and beliefs which can tidily be summarized, the temptation is to throw in the towel and, if one wants to talk about more than emigration statistics (and the article by Friedl is about emigration), to start talking about individual actions on the basis of some simplistic maximising 'psychological' model.

Not, of course, that the Mediterranean world lacks its institutions, institutions of some magnitude: the Catholic and Orthodox churches, for example. And they have a stake in such areas of traditional concern as kinship and marriage. But their study presents fresh problems for the anthropologist. Compadrazgo, 'god-parenthood', or in Greek, koumbaria, has attracted considerable interest. In what I consider to be a good essay, Stephen Gudeman (1971) has attempted a general analysis of the institution, and returns to it in a later essay (1975). Sensibly enough, Gudeman is anxious to get away from a purely 'functional' account, from the reduction of compadrazgo to its contribution towards 'solidarity', the socialization of children, the provision of emotional satisfaction, inter- and intra-class cohesion, and the provision of aid, labour, ceremonial assistance, housing, and so forth, for god-parenthood is not simply an idiom for these

functions; it has its own irreducible content. To understand this, the village perspective must be abandoned, and Gudeman turns to Church doctrine and history, tracing godparenthood from its first mentions in church writings, through St. Augustine and the various Councils and Synods up to its present fixing in the Catholic church in 1918. Despite a development which was not 'unilinear', he concludes that 'the complex has always been based upon the ideas of spiritual rebirth, spiritual paternity, and consequent spiritual relationships (1971:54). In short, there is a fundamental distinction to be made between the 'spiritual' and the 'natural' person.

It is in the third section of Gudeman's essay where he seeks to apply this distinction between the 'natural' and the 'spiritual' to his field-work data that I become a little uneasy.<sup>6</sup> That the baptismal set reduplicates the biological family is virtually self-evident; but that such a difficult theological notion as the distinction between the 'spiritual' and the 'natural' informs the thought of peasant villagers seems to me rather more dubious. (And lest I sound condescending, let me state that though in one sense I can follow church doctrine on the matter, I certainly find the distinction far too difficult to comprehend. I might adhere to it as an act of faith, but I cannot understand it). It should be noted that, 'the peasants themselves do not explicitly recognize this model' (1971:57). Rather, it is seen 'in the way they have "thought out" their social relations. . . .' (ibid). But I very much fear that what enables Gudeman to see them 'thinking out' their social relations in terms of a 'spiritual/material' opposition is precisely his knowledge of church doctrine, a knowledge which the peasants do not have. Once again, if the 'spiritual/material' opposition is a collective representation, it is a collective representation of a rather particular sort. Of course it can be argued that the nature of the liaisons which may exist between collective representations and the apprehensions of individuals has always been a difficult question and that, further, on the analogy of language, there is no need to argue for explicit knowledge in order to maintain the existence of such collective representations as ordering concepts. But at least in the case of relatively closed societies, one can assume that such ordering concepts as one discovers are integral to the society whose social relations one is observing. Though I think we would all agree that the nature of god-parenthood cannot be fully understood or 'explained' by studies at the village level, and that a recourse to history and to the great traditions of the church as a whole is thereby necessary, the problem which seems to me to have been avoided is just how, and on what authority, one reintegrates knowledge derived from these sources back at the village level. An ideology recovered in one area does not necessarily inform the practice of another area, even though the existence of that practice cannot be explained without it.

Here we face the second major problem of working with 'non-primitive' societies; for not only do their institutions lack the gross differences from our own which allow easy comment; they also lack integrity. We are not dealing with discrete, bounded societies whose collective representations can be understood by reference to the institutions of that society, or by their inter-relatedness to each other. This precludes not only the simpler forms of 'functionalist' analysis, as Gudeman well understood, but also many of the varieties of contemporary 'structuralism'. In fact I do not think that the 'openness' of most European societies is a separate problem from their not . . . being sufficiently 'alien'. Both problems can be seen to relate to the notion of 'cultural translation', which implies not only that we should be able to encounter something requiring translation, but also that

there exist (at least) two discrete 'cultural languages' or 'codes'. It has always seemed to me a rather simple point of elementary logic that if one is going to employ the linguistic analogy for cultures as a whole and claim that what should be analysed are not entities but relationships, and that 'meaning' resides in the 'system', then it is a necessary prerequisite for such analyses to be able to specify the bounds of the system within which such relationships are formed. This we cannot do. As a matter of fact, the problem obtains in linguistics itself. Chomsky, for example, has to maintain the necessary fiction of an ideal speaker-listener whose 'competence' bounds the language. And if one wants to keep with the fashion of linguistic metaphor, then I would say that the problem of working with European societies is rather like the problem of dealing with 'creole' languages. Of course there are 'systems', of course there are 'orders', and 'codes', but their encompassing structure is not stable, and systems and codes slide past each other and refuse to be frozen into a monolithic block; into a culture.

Let me give a trivial example from my own field-work. Following the church wedding, there is an interesting piece of ritual in the village where I stayed. The bride and groom repair to the bride's house where they eat, drink, and dance for a short time with a small number of guests. The bride then takes final leave of her parents, and with the groom goes to her in-laws' house, where she is greeted by them at the door. She is embraced and given an axe with which she crosses the thresh-hold three times; she is then given a pomegranate with which she does likewise before throwing it through the open door, she is then given a loaf of bread with which she again crosses the door-way three times; finally she is given a glass of water from which she sips, and which she then throws over her shoulder onto the crowd of onlookers behind.

Naturally, asking people what this means doesn't get one far. One old woman told me that 'Iron makes a strong marriage', but everyone else shrugged their shoulders, and though they were keen to point out the ritual to me as an example of a practice unique in their village (I very much doubt that it is), all they would say about it was, 'Well, that's the way we do things here', or, 'That's the way it's always been'. I was not so naive an anthropologist as to have hoped for anything other; that form of response appears to be one of the few universals in anthropology. But how successful can one hope other interpretative tactics to be? When the on-lookers to that wedding consisted of university professors and schoolteachers as well as illiterate shepherds, and when the bulk of the men of the village had sailed, as seamen, quite literally from China to Peru, can one make a 'structural analysis' of the ritual which relates it to other 'codes' in their 'social system'? I think not. The repetition of the triple-crossing, thrice times three, is connected of course with a variety of church ritual which reiterates the motif of the holy trinity. But pomegranates? Water? Inside? Outside? Pomegranates, seeds and fertility; bread and water, the basic necessities of life; a gesture of entering the house; a gesture of renouncing ties outside. I can think these up; so could the villagers if pressed. (And we can say similar things about throwing rice at our own weddings; they throw rice in the village too.) It seemed to me that what was important about this ritual to the villagers was precisely their realization that it was a piece of ritual, and their pride in it as an assertion of regional identity. The ritual itself was, if you like, bounded, and perceived as important as a unit; the society within which it took place, however, was not bounded, and I see no a priori reason to expect, and could



find no empirical evidence to suggest, that the elements of that ritual were to be found manipulated elsewhere in the thought of practice of the village -- except, of course, for the motif of the triple-crossing, and that is as pan-Hellenic as Orthodoxy itself.

Not that structural analyses and 'semiotic' approaches have not been tried in the Mediterranean world. David Gilmore has recently published an article on 'The Social Organization of Space: Class, cognition, and residence in a Spanish town'. Social class, he says, has long been considered as a 'structural principle in Mediterranean and Latin American communities', however, 'A Class model may be more than a principle of division by which people are placed in categories: it may also be internalized as a mental image or paradigm by which the universe of cultural and natural phenomena are ordered' (1977:437). Gilmore then seeks to show the 'generative role' of the class principle and its power as a 'perceptual framework' with respect, in this case, to 'social space'.

Briefly, the town of Fuenmayor has three relatively distinct socio-economic classes: the senoritos, the land-owners; the mayates, small-scale farmers; and the jornaleros, day-labourers. On the whole, the senoritos live in the fashionable centre of the town; the mayates live in a surrounding area, the periferia; and the jornaleros live in the barrio, an area on the outskirts with unpaved streets and inadequate sewerage, electricity, etc. With the exception that a good number of the wealthier middle-class, the 'strong' mayates, also live in the centro, and that the centro is not dead centre, this is all a matter of fact. But suddenly we find that in the interest of symbolic anthropology, this is all a 'perceptual framework', a way of 'ordering cultural and natural phenomena.' Thus we find that 'The barrios are (also) said to reflect a style of life associated with poverty and working class deprivation' -- scarcely suprising since everyone there is working-class, deprived and impoverished -- whereas... 'People tend to think of the centro as consisting of the seven well-paved and immaculate streets and the tree-lined plazas that radiate outwards from this oldest part of the town settlement.' And, 'Since the centre is thought to be the environment of the rich and powerful as well as the fount of political power, the labourers avoid going there' (my emphases throughout). And if one wants a 'symbolic confirmation' of all this, just turn to the cemetery where one finds that the senoritos have beautiful mausoleums and burial niches on a top terrace, the middle-class have well-kept burial niches on a second terrace (costing 500 pesetas), and the poor day-labourers are placed on a third terrace, untended and covered with weeds (and where the burial niches cost 250 pesetas, or nothing at all if one is buried at public expense). One can only be amazed at the lengths to which people will go to organize their conceptual universe.

#### IV.

Somewhat pretentiously I stated at the beginning of this paper that I thought Mediterranean anthropology raised some fundamental issues for the discipline as a whole, and perhaps I should attempt briefly to justify that remark (though I hope that some of my reasons have been at least adumbrated). I have argued that the fundamental reason why Mediterranean anthropology is floundering is that it is not dealing with societies basically different from our own. As a result, with some notable exceptions, too often it is teetering between the tedious and the silly. But at this point we should ask again why



this should be so. Some ideas have been already suggested. The first is this: that although we must admit in principle that societies similar to our own (or even our own society) are as susceptible to anthropological analysis as any others, as susceptible as the 'primitive societies' with which anthropology has traditionally dealt, in practice a radical difference appears, and we find ourselves 'translating' phenomena which are quite comprehensible already in terms of our own 'common-sense' assumptions about the nature of the world into some sort of 'anthropological language,' which, if taken as an ethnographic account of the peoples we are studying, makes them appear from our own strictly relative point of view to be very much odder than they are. Inextricably connected with this is the fact that in the absence of what I have called 'gross institutional differences' there comes a genuine crisis of 'what to say' about Mediterranean societies. There seems little point in noting the brands of soap-powders used by villagers. It is this situation, I think, which forces Mediterraneanists to explore those areas which are among the most cloudy and unsure -- collective persuasions, attitudes, sentiments -- or at times to clothe the common-place in a prolix language of pseudo-analysis.

Perhaps all this is only to say that Mediterraneanists are faced with a rather more difficult task than their predecessors who worked in other areas of the world, and that, when the concern is not with the (very necessary) compilation of 'hard' sociological data, a much greater emphasis needs to be placed on the ethnographer's almost novelistic skill to recreate and convey those subtle but cumulative differences which certainly do exist and which certainly do give to Mediterranean life a particular quality. But if this is so, another question, or at least a suspicion, is raised. Could it be the case that, although almost anything said about an unknown and alien culture was bound to hold some intrinsic interest because it was alien and unknown, the analytic manipulations performed on it also seemed more acceptable just because one's common-sense view of the world was deemed to be inoperative? Would, for example, something analogous to Gilmore's argument about 'class cognition' and 'social space' be more acceptable if it was located in the depths of the Amazon jungle or in 'darkest' Africa?

These are questions which I certainly cannot answer; but one of the things which has consistently worried me in working with Mediterranean material is the notion which, in one form or another, I think we all carry round as part of our professional baggage, and which I think is implicit in almost all theoretical stances, and which is certainly integral to the predominantly relativistic view-point we possess: namely, that cultures are bounded entities, that we encounter total 'social systems', that we can speak of the 'collective representations' of the so-and-so, and that there are discrete universes of 'socially constructed reality' and nicely intact 'world-views'; in short, that as anthropologists we can throw a net around the thoughts and practices of a collectivity. This is, I know, a rather old question, but it still seems to me to be a rather important one, and the fact that so many attempts to circumscribe or lay-bare the lives of Mediterranean communities in terms either of some form of structural analysis or some investigation of basic categories of thought appear (to me, at least) as rather unsuccessful, whereas, in other more exotic contexts, similar enterprises did not, may mean that wittingly or unwittingly Mediterranean anthropology's contribution to the discipline as a whole will be in terms of promoting some careful reconsideration of the very notion of 'a culture' or 'a society' which we so readily, almost instinctively, employ. For it was the dissolution of a radical distinction between 'them' and

'us' in terms of 'primitive' and 'non-primitive' which allowed the advent of a European or Mediterranean anthropology in the first place, it is nevertheless the maintenance of a radical distinction between 'them' and 'us' in terms of cultural boundaries implied by the very search for a society's set of 'collective representations', its 'social system' or its 'cultural code,' which impedes us now.

Let me end with an anecdote. My host, 'Bill', owned a cafe-bar in the village, newly constructed out of concrete, bricks and mortar. One day he decided it needed a new exterior window in a side wall, and disappeared by caique to the town, whence he returned with a complete window, already primed and glazed. The policeman strolled past, and Bill beckoned him in to hold the window up against the wall. Bill then quickly drew a pencil from his pocket and traced the outline of the window on the wall. I was still trying to think of the words in Greek for 'tape-measure', 'set-square' and 'plumb-line' when a sledge-hammer was placed in my hands and I was told to knock out the hole. We jammed the window in. Credit where credit is due - it was only about an inch down on the left-hand side - but I could not help feeling that I had just participated in about the shoddiest piece of workmanship I had ever encountered. Then it suddenly occurred to me that had I just witnessed someone erect by eye a grass hut or a wind-break somewhere in Africa or New Guinea, rather than feeling put out by the absence of tape-measures, set-squares or plumb-lines, I should probably have admired the native skill and even written about the process at some length. It was the bricks and mortar which were misleading me. Things were different here, but I was judging them as if they were the same. But then it also occurred to me that by saying that I too was erecting a sort of window; a glass-wall which guaranteed that what lay on the other side had its own rules which were not mine.

Roger Just

NOTES

1. Cf. Needham, R. 1973.
2. Various 'non-primitive' societies studied by American and British anthropologists prior to 1950 are listed by Evans-Pritchard (loc.cit).
3. Loizos 1975 and 1975a.
4. It is worth noting that by far the most interesting book on Mediterranean kinship is that by Hammel (1968), which was largely concerned with Yugoslavian material; and in Yugoslavia one does (or did) find an institution of considerable 'difference' — the zadrugas, kinship groupings which were also corporate bodies with respect to land-holding etc. and, importantly, with respect to god-parenthood ties.
5. The approach is credited in a foot-note to Barth and Boissevain, the best known 'transactionalists'.
6. In fact Gudeman's field-work was conducted in Panama, not the Mediterranean — but the problems are the same.

REFERENCES

- |                 |      |  |
|-----------------|------|--|
| Ardener, E.     | 1971 | 'Introductory Essay' in <u>Social Anthropology and Language: ASA Monographs 10</u> London: Tavistock (ed.) Ardener E.                              |
| Beidelman, T.O. | 1977 | 'As we see others' <u>Times Literary Supplement</u> June 17, 1977, p. 741.   |
| Blok, A         | 1974 | <u>The Mafia of a Sicilian Village 1860-1960.</u> Basil Blackwell, Oxford 1974.  |
| Campbell, J.K.  | 1964 | <u>Honour, Family and Patronage: A Study of Institutions and Moral Values in a Greek Mountain Community.</u> Oxford University Press: Oxford 1964. |
| Chapman, M      | 1978 | 'Reality and Representation', <u>Journal of the Anthropological Society of Oxford</u> Vol. ix, No 1., Hilary 1978 pp. 35-52.                       |
| Cutileiro, J.   | 1971 | <u>A Portuguese Rural Society</u> Clarendon Press: Oxford 1971.  |
| Davis, J.       | 1969 | 'Honour and Politics in Pisticci' The Curl Prize Essay. <u>Proceedings of the Royal Anthropological Institute</u> 1969 pp. 69-81.                  |

- Davis, J. 1977 People of the Mediterranean  
Routledge and Kegan Paul: London, Henley  
and Boston 1977.
- Douglas, M. 1975 Implicit Meanings  
Routledge and Kegan Paul: London and  
Boston 1975
- du Boulay, J. 1974 Portrait of a Greek Mountain Village  
Clarendon Press: Oxford 1974.
- Evans-Pritchard, E.E. 1951 Social Anthropology  
Cohen and West Limited: London 1951
- Friedl, E. 1976 'Kinship, class and selective migration' in  
Mediterranean Family Structures (ed.) J. G.  
Peristiany, Cambridge University Press: Cambridge,  
London, New York, Melbourne.
- Gilmore, D. 1977 'The Social Organization of Space: class, cognition,  
and residence in a Spanish town' American Ethnologist  
Vol. 4 no. 3 (August 1977) pp. 437-451.
- Gudeman, S. 1971 'The Compadrazgo as a reflection of the natural and  
spiritual person'  
The Curl Prize Essay 1971. Proceedings of the  
Royal Anthropological Institute 1971 pp. 45-71.
- Gudeman, S. 1975 'Spiritual Relationships and Selecting a Godparent'  
Man Vol. 10 no 2. (June 1975) pp. 221-237.
- Hammel, E.A. 1968 Alternative Social Structures and Ritual Relations  
in the Balkans  
Englewood Cliffs (Prentice-Hall) 1968.
- Loizos, P. 1975 The Greek Gift. Politics in a Cypriot Village  
Basil Blackwell: Oxford 1975.
- Loizos, P. 1975a 'Changes in property transfer among Greek Cypriot  
villages' Man Vol 10 no 4 (Dec. 1975) pp. 503-523.
- Needham, R. 1973 'Prospects and Impediments' Times Literary  
Supplement July 6, 1973 pp. 785-786.
- Needham, R. 1976 'Skulls and Causality'  
Man Vol II no 1 (March 1976) pp. 71-88.
- Ovesen, J. 1978 'Maurice Godelier and the study of Ideology'  
Journal of Anthropological Society of Oxford  
Vol. ix, no 1. Hilary 1978 pp. 1-11.

PRATTLE AND POLITICS: THE POSITION OF  
WOMEN IN DOSHMAN-ZIARI

In the world-wide debate on the position of women, Middle Eastern anthropologists have been attempting to discern the role of women in societies with a predominantly 'patriarchal' ideology, in which, it is assumed, women have no influence in economic and political decision making. Most of the ethnographic studies on the Middle East have been made by men, and, in Islamic societies in particular, it can be suggested that their ~~access~~ to information from women has been restricted, and that their understanding has been shaped by the male perspective. C. Nelson in a review of the literature makes a similar point and then proposes that:

From the ethnographic literature on nomadic society there is ample evidence to support the idea that the woman defines herself and her position in terms of values centred about the man. She then uses the male centred value system to attain her own ends by way of manipulative techniques that force man to recognize female power without losing his self esteem (C. Nelson 1973:56).

While Nelson's use of 'manipulative techniques' and 'female power' implies a female conspiracy, her suggestion of the pivotal role of the value system between the position of the two sexes is important. From my material collected during fieldwork<sup>1</sup> in one section (ta'ifeh) of the Doshman-ziari division of the Mamasani tribe in Southern Iran, I would like to advance on Nelson's position by arguing that two models of society are being used, and are connected by the common adherence to a set of concepts. The clarity of the dual models centring on a common interpretation of these concepts is becoming clouded with the changes that are occurring with the introduction of teaching as a profession for both men and women. However, the present paper is limited to a discussion of the traditional structures which are still evident in the behaviour and attitudes of the majority of the villagers. One model of society is used by both sexes, especially when addressing outsiders, and can be termed the 'dominant model'.<sup>2</sup> The concepts which structure that model are accepted by the women, but by employing the obverse implications of their meaning and their interlinking, women obtain a degree of freedom of action and influence in social affairs from which the dominant model theoretically excludes them.

During several conversations with both male and female informants the attitude was often expressed that male children are much preferred, and the birth of a daughter is not a cause for celebration. The explanation given by male informants for this preference was that 'sons always stay with you, daughters leave'. Women on the other hand often remarked that their married daughters kept much closer contact with them and helped them more than their married sons. This was an obvious indication that while both men and women might express the same attitude, their stances might differ.

Men often validated the above statement by saying that as residence is patrilocal (that is, on marriage a son will build his house in his father's yard), and as rules of inheritance exclude women, men stay closer together, have common vested interests, and will support and protect each other. In contrast a woman may join the household of another agnatic set on marriage. One man explained this attitude more fully by saying that while all children are called descendants, sons are classified as oulād. My informant said that

the strength of a man is related to the number of members in his oulād. By this he was referring to the intensely competitive atmosphere in the community wherein, in order to survive, a man has to gain a reputation for his ability to defend himself and his possessions. The aridity of the land and the scarcity of resources mean that if a man wishes to retain his holdings (his land, vineyards, and herds), utilize them to best effect, and if possible expand them, he has to have a dependable labour force. Moreover, where political strength is still on occasion assessed in terms of the number of males a man can gather to fight for him, sons again can be depended upon for support more than any other relative or friend. Once a man has gained respect from the community he will then be valued as a political supporter, or might even be able to attain such a position of influence that he can gather a sufficiently large personal following to form the core of a factional grouping. With this in mind, my informant, only half-jokingly, said that 'a man who has five sons can become king'.

Within the prevailing conditions of competition, the oulād, the set of male siblings, is the only group within which it is possible to create an atmosphere of cooperation, confidence, and discretion. Male siblings try to work cooperatively. Even after the father dies, the brothers try to act together, retaining their father's holdings in common and presenting a united front to the community. Cooperative exploitation of resources used to be much more common than it is now, as none of the traditional economic activities (agriculture, grape production and animal husbandry) would alone provide a sufficient income. The economy therefore had to be mixed, and the most efficient method of attaining this was to divide the labour between brothers who held all the resources and the income in common. The eldest brother would then try to be a surrogate for the father, organizing the economic activities, the division of the income, and acting as guardian for his unmarried siblings. Nowadays, the expansion of the market economy along with low prices gained from the sale of food products, and the high rate of inflation, has meant that traditional economic activities have been devalued. People are beginning to look for a personal annual cash income they can invest, in trading or transport, or are leaving agricultural activities altogether to gain a salaried post as a teacher or gendarme. However, while joint economic enterprise between brothers is declining, it is still understood that if an elder brother publicly states an attitude or position it can be assumed that his brothers will concur. Thus the oulād is still considered a united group in political affairs and in the context of community decision making, even if the brothers' economic interests might be diversifying.

A man would not therefore expect his brothers to break the confidence and discretion obtaining between the members of the oulād. He would expect his brothers not to tell anyone about his affairs, reveal his interests, or disclose any information about the siblings which others could use to belittle or defeat him.

While oulād refers to the sons of a man who is still alive or who has only recently died, the word oulād can also be used for the male descendants of a man who was alive two or three generations ago. This is merely an extension of the meaning of the word. Agnatic cousins are the descendants of brothers who in their own time were sharing the greatest cooperation and confidence. There are very few descent groups of greater depth than three deceased ancestors, so by using brother in a metaphoric sense, oulād can generally refer to agnates.

Thus, after the sibling set, it is among agnates that the next level of confidence and solidarity can be hoped to be achieved. Beyond the agnatic set no confidence or solidarity can be depended upon, and very often it is accepted that between men who are not agnates there can rarely be more than distrust and rivalry.

Therefore, a man in search of security in this competitive society must attempt to maintain solidarity between his male siblings, then between his agnates, and then try to gain a following of others who look to him and his oulād for protection and who, by giving him their support, promote him to a position of influence within the community.

However, while all will applaud the ideal of oulād solidarity, it is precisely within the oulād that the greatest competition over resources can occur. While the eldest brother is acting as surrogate for the father, his brothers might feel that the division of income is unequal. After the division of the holdings, which is made with elaborate precautions to ensure equality, if one brother makes more of a success of exploiting his resources than another, claims will be made that the division was unfair. Even if a set of brothers can resolve internal differences, there are pressures from outside which produce friction. A set of brothers who can work effectively together will rise in prestige and gain political followers. They will then be a target for attack by similarly constituted groupings. An opposed group will sow discord between the brothers, and try to attract one brother into their camp, thus splitting the oulād and weakening it. When such a situation arises, others will say that the offender is 'not behaving like a brother', again referring to the ideal of fraternal solidarity.

The most important means by which they try to maintain oulād solidarity and attract the confidence and support of the other agnates and followers is by the choice of marriage partners for their agnates and their female siblings. Indeed, when an important oulād declined in importance in the village, it was said that this was 'because they had not sold their women wisely'. From a study of the most successful sibling sets, a pattern emerges from the distribution of their marriages.

Most often the eldest and most able member of a set of agnates will be married to the sister or daughter of a man who is more influential than himself. He will then support his father-in-law, in the hope that his father-in-law will protect his interests. Such protection from an influential man will then hopefully attract other men to him for protection. Then each of the sets of his agnatic cousins will usually be interlinked by marriage. If there are three sets of cousins, then three FBS-FBD marriages will effectively tie all the sibling sets together and reinforce the ideal of agnatic solidarity with affinity. The remaining brothers and sisters will be married to outsiders. These outsiders will either be people of the same standing as themselves who have interests in common with them, or people weaker than themselves who are looking for protection and from whom this agnatic set can expect support. Disaster begins to set in if one of these marriages to an outsider is to a man who becomes more influential than themselves, as they have then engaged in two such marriages. As a result the two sets of influential in-laws will both require support and might well be pulling the agnatic set in two different directions, thereby putting strain on the ideal of agnatic solidarity.



The choice of marriage partners is thus a constant preoccupation. There is a need to make an appropriate selection of in-laws which provides a balance; if one is more influential and offers protection, and others are less influential and offer support in return for protection then the agnatic set can maintain its solidarity and rise in influence within the community. If the selection of marriage partners, with time, does not provide such a balance of affinal ties, then not only does the strategy for gaining influence fail, but the position can be reversed, and the brothers themselves can be split by their mutually incompatible affinal alliances.

Thus from one point of view, the society can be seen as constituted of groups of males professing solidarity, but riven with potential frictions and hopefully held together by the careful choice of marriage partners, especially for their women. Women are excluded from membership of the oulād, which is a male unit, and are the responsibility and under the protection of their eldest brother. Thus according to this model, women do not stand as independent beings.

Complimentary to the idea of oulād, or perhaps on occasion in contradiction to it, is the idea that 'milk is dear'. That is, those who shared the same milk are cherished and close to each other. Thus while the idea of oulād emphasises exclusively male membership and solidarity, the idea of milk-relatives ties members of both sexes into a matrilineage. The mother is treated with great respect even by her married sons, and often she acts to keep her sons and their wives united after their father has died. It is generally believed that sons who have mistreated their mothers will find that their life is not acceptable when they reach the afterworld. Perhaps the fact that sanctions for good behaviour towards one's mother are only enforced in the afterworld may be taken to indicate that the links uniting the matrilineage are weaker than those of the oulād. While matrilineage relationships are often called 'dear' and 'close', I never heard matriline associated with the idea of solidarity, which was expressed very frequently as one of the ideals of the oulād.

However, while milk-relatives are often talked of in affective terms, in practice men might look to their MBS or ZS for cooperation in a joint economic venture--and this is not always because their own agnatic set is small or weak. Indeed if we return to the oulād, and the sensible distribution of marriages for an agnatic set, the marriages with outsiders, and especially with those who are of equal standing, are often with matrilineal kin. Thus, in the model based on the oulād, the choice of marriage partners assumes some importance for matriline, even though it might not be clearly stated as such.

One of the reasons why contact with the mother's relatives is maintained is that women make the initial arrangements for a marriage. In the selection of marriage partners, just as the men are acting on considerations about how to maintain their economic resources and exploit them effectively, the women are faced with the problem that many of their domestic tasks need cooperation between two or more women. An isolated woman is faced with great problems. She cannot find a baby-minder when she makes her daily trip to collect water or when she does the washing in the stream. The weekly tasks like making bread, or the jobs of feeding her husband's guests are much more efficiently accomplished when done cooperatively. Thus, as residence in the village is mainly patrilocal, so that the village consists of clusters of houses of agnates, and as visiting between women is usually between neighbours, the desire of the women to stay close together is concordant with the idea from the oulād model that some marriages should be with agnatic cousins. Beyond

this the alternative marriage pattern, which would not leave the woman isolated, is with her maternal kin.

Thus when the women are making the first overtures for a marriage between their children, they have two considerations in mind. A wife knows where her husband's interests lie and what would be an acceptable marriage to him in terms of his economic and political designs. Secondly, a mother ensures that her daughters stay near her, and near each other, and that contact is maintained with her own kin. While a marriage between matrilineal relatives might be seen as such by the women, a man might consider a marriage between his daughter and his wife's relative as an affinal link with another set of agnates to establish the possibility of closer cooperation in economic and political affairs. Thus a marriage between two matrilineals is also a marriage between two oulāds. In this way, a mother is initiating a marriage tie in terms of her own interests, but her perception of her husband's interests usually makes the match acceptable to him, although he might have a different interpretation of the nature of the link.

The complication of this duality between the ideas of oulād and of milk-relatives is seen in the position of a woman on marriage. She is not a member of an oulād, as that is constituted only of males, but she is a member of a sibling set. Moreover, she does not lose that status on marriage. When she marries, she is joined to her husband's descent group, but she never breaks her ties with her natal group. This can be seen most clearly from the fact that even after she has had children, her brother is responsible for her protection, and if she behaves badly her brother's name, as well as the name of her children, is blemished by her act. It is just these dual ties of a woman which underlie the importance of marriages as a way of making alliances. A woman is the pivot between two descent groups--hopefully pulling them together. However, it is also this which makes the position of a married woman difficult, especially when she is married to a matrilineal relative of equal standing to her natal group. Her husband wishes her to attract her brothers into his support group while her brothers want her to draw her husband and his brothers towards support for themselves. She is between two oulāds, and is a member of neither, but associated with one by birth and the other by marriage. Her position is thus ambivalent. The strains on the oulād from the latent competition between brothers, and from threats to its coherence from outside, are augmented by the ambivalence of the position of a married woman. She has knowledge of the affairs of both oulāds which are both ideally the focus for secrecy and discretion, yet she is expected by both her brother and her husband at the same time to exercise discretion over the affairs of their own oulād, and to 'leak' on the affairs of the other. Thus by the nature of her position in terms of the ideal of the oulād she must be considered 'irresponsible' and 'unreliable'.

While her position between two oulāds leads a woman to be considered irresponsible and a source of discord, her position in her husband's descent group has the same effect. Women are blamed by their husband's brothers for the ultimate defeat of the ideal of fraternal cooperation, when they finally decide to divide the holdings inherited from their father. When brothers decide to establish their households as separate economic units there is a great disquiet, as the material base for cooperation between brothers is thereby removed and a new relationship of cooperation and trust has to be established between them on a less concrete foundation. Women are blamed for creating this situation.

One man explained the rationale behind this attitude by saying that whilst all his brothers were unmarried their interests were concordant, but when they all had wives and two or three children their interests diverged. Each brother became concerned with his own household and the interests of his children. One brother had fewer children than all the others so his consumption was less, and

he wanted cash for an investment which the other brothers could not afford. Then he had to choose between maintaining solidarity with his brothers against the interests of his own household, and dividing their property so that each household could manage their own affairs in the best interests of their children. Thus the wife becomes the key element in the contradiction between these two ideals. On marriage she is the member of her husband's descent group with the weakest ties to the oulād and she is only gradually bound to it through the birth of several children.

However, it is just the birth of these children which causes her to be the pivot around which the husband turns from his concern for oulād solidarity to a desire to act in the best interests of his own children, and thus to create a new oulād. Thus in the context of the oulāds, both as a sister and as a wife, a woman is necessarily in an ambivalent position, and will be considered 'irresponsible' and 'unreliable'.

Following the internal logic of the oulād model, if women are necessarily unreliable, the areas in which women can operate and be influential should be restricted. In this context, one often hears that 'women have no ekhtiār!'. This word has a multiplicity of meanings, but probably the best translation is that women have no choice or right to independent action. Complementing that phrase is the assertion that 'a woman's ekhtiār is in the hands of her husband'. Thus the husband takes responsibility for all affairs concerning the public face of the household, and his wife is theoretically excluded from exercising any responsibility or independent judgement in economic and political affairs. These are denoted as male domains; the wife's role is restricted to that of running the household, and she is thus theoretically restrained from acting in public in a way contrary to the interests of her husband and his oulād.

The idea that women have a restricted role in society is seen in many behavioural patterns. Within the household, when male guests arrive, the man acts as the public face of the household while the women remove themselves from the room and prepare tea and food outside. Often the women will not enter the room at all, and will engage a younger male relative to carry in the tea and serve the meal to the guests.

In the household division of labour the same impression is given. All the factors of production and all work and decision-making concerning the earning of the income are in the hands of the husband. Once the products have been brought to the house they come under the control of the wife. She is responsible for processing the agricultural and animal products, she can decide whether products set aside for domestic consumption can be given to neighbours in need, and she tells the husband in no uncertain terms what he must buy on his next trip to town. The collection and use of firewood can exemplify the demarcation line between the man's work outside the house and the woman's work inside the house. Men collect the firewood from the forests, and also break it up and stack it near the house ready for use. Women use the firewood, and if they find it insufficient or not broken up can refuse to cook bread for the household.

Thus the wife is in complete control of the domestic affairs of the household, including the upbringing of the children until they are of an age to move around independently outside the house and the yard. Meanwhile, the husband will only in periods of extremely intensive labour request assistance from his wife in any of the activities outside the house and the village. It is entirely his responsibility to provide an adequate income for the household.

Thus there is a demarcation of areas of responsibility: while women are not expected to engage in public affairs, men are not expected to interfere with the women's running of the households. Perhaps this latter aspect can be most clearly indicated by the fact that when the husband is at home, he occupies the place in the house on the side of the fireplace farthest from the door, which is on other occasions where guests are seated.

Although such behavioural patterns give the impression of a division between male/public and female/domestic, the oulād model demands more than this, for, if the woman is to be prevented from exhibiting the irresponsibility and unreliability inherent in her ambivalent position in the oulād model, then she should be prevented from having any independent social activity. That is, a woman should not be a social being. Even in societies where women are physically restricted by being in pardah this degree of seclusion from social activity cannot be achieved because the women still have their own kin, and concomitantly their own interests and desires, which at the very least they can pursue through contact with other women.

In Doshman-ziāri women are not so physically restricted. Rather, it is the men who, during the agricultural seasons, are isolated on their own land outside the village. The women stay in the village and are in touch daily with their female kin and can come into contact with unrelated males -- travellers, visitors, and village men who have not gone to work. The oulād model requires a degree of male control of the public activities of their female kin which cannot be achieved. The wife's ekhtiar can never entirely be in the hands of her husband: she is left with a responsibility not to behave irresponsibly. Among all the areas of social activity, the one where a woman can most clearly demonstrate this shortfall between the logical implication of the concept of male responsibility and the practical limitations of male control, is in sexual matters. Here a woman can most forcefully exhibit her independence and accordingly her 'irresponsibility' and 'unreliability'.

Thus the structural position of women in the oulād model seems to accord with the male opinion about woman's sexual nature - that she is prone to irresponsibility and deceit. From a functional point of view, a man's repeated homilies to his female kin enjoining them to protect their sexual shame (nāmus) might be seen as a wish to control woman's child-bearing capacities to maintain the clarity of descent in the oulād. However, the concern expressed by men seems to refer more to the nature of women, and especially to their sexuality.

One informant elaborated this attitude by telling me the story of Eskandar. Eskandar lived with his mother, and as he grew up he became aware that all women are deceitful. In answer to statements to this effect, his mother would reply, 'Yes, all women but me'. One day Eskandar left home, and quite some time later he returned disguised as a Darvish.<sup>3</sup> He knocked at the door of his mother's house and said that he was very thirsty and hungry. He persuaded his mother to let him in, and sat down and engaged her in conversation and stayed for dinner. When he was still there by night-fall, the mother was forced to ask him to sleep there, and she laid out the bedding either side of the fireplace. He continued to chat to her, until he finally persuaded her to lay out the bedding side by side. At this point he revealed his identity and said 'Yes all women are deceitful, including you'.

This story was taught to my informant as a young boy at the village religious school, and not only demonstrates that women can never be trusted, but also indicates the difference between the status of men and women. Eskandar was the one who, in our terms of reference, engaged in deceit by disguising himself and tricking his mother. However, when I maintained that his behaviour was at fault, I was informed that his behaviour was not unreasonable because 'men are free'. It was the mother who was being deceitful because she allowed the man to enter her house when there was no man there to entertain him. She entertained him herself, and, if this was not sufficient transgression, she at the end made it obvious that she did not protect her sexual shame.

Even after the marriage of a girl, her brother, father, and husband will continue to insist that she protect her nāmus: since the woman has no ekhtiār, her male relatives are responsible for her, with the corollary that if she misbehaves her actions affect their reputation. While illicit affairs and adultery are the ultimate sign of a woman acting independently, in practice it seems that the incidence of such affairs is low. However, the implication behind the demand that women protect their nāmus is that they should behave in such a way that not the slightest hint is given of any inclination to behave in an inappropriate manner. Thus the woman's behaviour is prescribed. She should be modest and demure. She should be quiet and not engage in quarrelling with the neighbours; she should be deft at completing the household tasks; and should be kind yet unobtrusive in the way she treats guests. In every aspect of a woman's behaviour nāmus is at issue and she should demonstrate her good character through her retiring behaviour in public.

Such demeanour is usually associated with the wearing of the chādor, a cotton cloak which covers the head and goes down to the ground. But this is usually only worn by women when they are moving outside the territory where their male kin live, and especially when they are visiting another village or the town. This cloak veils the entire body, and besides that, the woman holds one side of the chādor across her face and averts her face to that side, so that men cannot see her profile. Within the village, although the chādor is not usually worn, a woman, in the course of her work to collect water, wash clothes at the stream, or visit the trading shop, has to pass through alleys where non-relatives live. Then, if she passes a man, she similarly averts her face. If a man addresses her, wanting news or information, she averts her face, drops her head, answers quickly and quietly, and moves on.

Any consistent failure in her character or behaviour will invite the comment that she does not protect her nāmus. Moreover, such comments would more often be made by a woman than a man, as it is the greatest insult for a man to refer to the nāmus of another man's women-folk. Such an attack by one woman on another in the course of a quarrel indicates great tension in the relations between the households. It invites an escalation of the conflict, since, if it is taken seriously, an attack on the woman brings her brother in to support her husband and thereby protect his sister. Indeed, during the war which preceded my fieldwork, one man had persuaded the political faction he belonged to not to attack a certain house because 'his nāmus was there'. A man does not have nāmus; what this meant was that his daughter was the wife in that house, and he was trying to protect her nāmus in order to maintain his own respectability and reputation.

Thus, while a woman might be considered to have no right to independent action (ekhtiār), the obverse of this is that the reputation of her husband and especially of her brother and sons rests on their ability not only to control her, but to defend her. Making reference to the very beginning of the paper, this is the reason why women prefer to have sons. A mother without a son, or a sister without a brother has no one to defend her. A woman who has no protector is open to attack, but one who has a male relation with a good reputation knows that she has much greater freedom of action, as the brother or son has to defend her to protect his own reputation.

While ekhtiār is theoretically in the hands of a woman's male kin, for the woman this has the practical implication that any accusation of misbehaviour levelled at her, rebounds on their reputation. Their reputations and hers are interwoven. The woman can therefore expect her male kin to defend her, in the defence of their own reputations. This gives the woman a certain freedom of action as a social being.

The reputation of a man is vested in his ābroo. The basis for a man's ābroo is the size and strength of his oulād, but beyond that, his own personal character and acumen are taken into account by the community, as are those of his wife, in the assessment of a man's social standing. A man has honour and a certain level of standing in the community, and while this can be increased over time, it is the 'loss of ābroo' which is most often a source for concern because it can happen as the result of one encounter. Ābroo can be lost by a man who is placed in a situation where he is belittled by others. Ābroo is thus at the centre of the competitive atmosphere of the community. But while the man can lose ābroo through his own actions or those of his adult son, his ābroo can also be lost by the actions and behaviour of the women for whom he is responsible.

Thus while the woman might have interests in complying with the behaviour stipulated by her husband or brother in order not to put her nāmus and his ābroo under attack, equally her husband or brother might be persuaded to act in a way which complies with her interests in order to protect his own ābroo. Here we are beginning to consider the obverse of the oulād model. If a woman has no rights, and her reputation is intertwined with that of her husband and brother, she has greater means to manipulate her menfolk, albeit with great tact, than their presentation of the situation might imply. It is just this room for manipulation which allows her to take advantage of her ambivalent position in the structure of the oulāds, not only to work for the interests of a husband against a brother or vice versa, but also to represent to them the interests of her matrilineage.

This implies that, as opposed to the formal position of the woman, being restricted to the household and excluded from influence in the male social activities of politics and decision making, she can have great influence - so long as she does not act overtly, as that would invite attacks on her reputation and bring her husband's ability to control her into question. This influence can even extend into the essence of the male domain: political decision making.

Community decisions are formally made in a series of male meetings lasting about a week. Decisions have to be made on any issue which involves more than one oulād: they might include such matters as to how to arrange the communal purchase of a tractor, how to avoid the resurgence of the war in the village, or how to resolve a quarrel that has arisen over the purchase of a cow. The



meetings take place between men after dinner when they have returned from their work. Debate on an issue usually starts informally, when a person who wants advice or assistance goes to the house of an influential man, and most evenings there will be gatherings in the houses of such men. If a situation develops into a crisis and a decision has to be made, the meetings then become formal, and one influential man invites others to dinner. The women are nowhere to be seen on these occasions, although they always manage to hear what is said.

The influential men are the most respected members of each set of agnates, and they act as spokesmen for their agnates and others who have associated themselves with them by marriage or economic ties. They gain influence by being able, through a series of meetings, to demonstrate their political acumen--their ability to keep unity among their supporters by defending their interests, and their ability to win over other groups to their own point of view.

Any meeting is between recognized men of influence, others who have an interest in the issue, and anyone else who feels he has a right to be there. Thus few men of very low status would attend as it would be taken as unwarranted forwardness, and they would lose face. These people depend on an alliance with a man of influence for the protection of their interests.

The meetings are ranked, with the man of greatest influence sitting by the fireplace on the further side from the door where the best carpet has been laid. Others sit in order, from him ranged round the front of the fireplace, to the door. Thus the men of lowest rank are seated with the draught from the door on their backs. On the opposite side of the fireplace sits the house owner and his brother or other close relative. They are effectively out of the pecking order whilst they are hosts, and are occupying the side of the fireplace where the wife sits when the family is alone. He also undertakes the tasks which are recognized as pertaining to the wife within the household when there are no guests: making the tea, cutting the sugar loaf into lumps, and serving the tea.

Both the seating order and the tea drinking order are ranked, and are the occasion for much formal etiquette. When a man enters the room, he pauses to remove his shoes. If he is important all will rise, and those who know they are of the same standing as the new entrant will offer him their place, and move one place round the arc. He will refuse and try to sit by the door. While he is crouched and about to sit down, others will grab him and pull him into a higher position. In contrast, a man of low standing will enter the room with subdued greetings and sit down immediately by the door. The other men in the room might return the greeting, or might ignore him completely, not even making eye contact. Nobody will stand up.

When tea is served, the host places three glasses on a tray and pushes it over the carpet to the man opposite him in the highest position. The man will then push the tray to the centre of the gathering and say to the company in general, 'After you'. Everyone replies, 'No, after you', and he then takes a glass and some sugar and pushes the tray to the man sitting next to him. This continues, each man repeating the procedure twice, and drinking two glasses of tea, until all have drunk.

In these assemblies, the ranking order of the males is established and reassessed. All will speak when they have something to say, but when two people wish to speak at once, deference is made to the man seated in the higher



position. If the man in the highest position wants to retain his place, he will say most. He, like every one else, will try to demonstrate his wisdom and his personal ability to understand the different interests and to reconcile them by tactical poses. The subject of his speech is only part of this demonstration of ability and wisdom. The manner in which he speaks is also assessed. He must ensure that he does not offend anyone present by making indiscreet reference to anyone's relative, and the purport of his speech should be revealed only by direct statements and the use of innuendo and sophisticated hinting. In these meetings, over time, the consistent exhibition of these abilities, or lack thereof, establishes a man either higher or lower in the ranking order, and with his position goes his prestige and reputation (äbroo). The form of communication between males, as well as the whole tenor of their meetings is formal and structured, and the ranking order is flexible, but public.

Women are not usually seen in this public debate. The next afternoon however, the women are gathered in one person's yard, engaged in spinning or other portable household tasks, and enjoying the sunshine. However, these gatherings only take place in certain yards: those where the wife of the household belongs to one of the biggest matrilineages, whose brothers form an influential agnatic set, and possibly, although not necessarily, those whose husbands are also influential. This means that the women are not always meeting in the yards of the houses where the male meetings are gathered.

In the women's meetings the seating pattern is not usually well organized, nor is there such an elaborate display of etiquette as seen in the men's meetings. The main difference from the male meetings is that if a carpet is laid out, the woman of the house will sit on it along with other women from important families who are recognized to have personal ability and understanding. Others sit in the dust. This is in great contrast to the form of the men's meetings, where the householder plays humble host. Also, unlike the male gatherings, the conversation will rarely be directly focussed on a certain issue. To an outsider it appears that the women are engaged in idle chat. However, in the course of the conversation, one of the women who is most respected will drop some oblique comment that, for example, the wife of a certain person cannot control her chickens and they are constantly excavating somebody else's yard. From this it can be gleaned that the two wives or their husbands are at odds. Issues either concerning the women themselves, or pertaining to the men's debate are therefore discussed in a very convoluted manner. Women are presenting their attitudes through inferences made in the course of a conversation about matters concerning the woman's domain. Thus their conversation will be about, for example, how the behaviour or character of another woman does not meet the generally accepted standards: that she is lazy or unable to fulfil a particular household task, or that she quarrels, or that her child misbehaves; or it will be about some other aspect of a woman's household responsibilities, and here chickens are most frequently the subject matter. Sometimes a large gathering of women assembles in the yard of a house where the male meeting took place on the previous evening. The issues that were discussed in the male meeting have to concern a clear division in the village or involve relations between the village and outside, before they are expressed in anything but a very circumspect manner by the women.

If I asked one woman directly what was happening, she would give me an account which indicated that even the least influential of the women had a full picture of the issues under debate. However, the women would rarely give an expose of this kind to each other. Their knowledge would be gleaned from sitting in a number of people's yards and, from a series of obscure comments on apparent non-issues, they would arrive at an understanding of the strains or accord

developing between different men and the attitudes of different women.

The subject matter and mode of discourse in the woman's gatherings add to the impression that the women are not acting overtly in the male arena. The character of their discourse seems designed to maintain the clarity of a division between the sexual domains: it is possible, too, that the very nature of the women's circles<sup>4</sup> demands such a complex mode of communication. Maybe the women's discussions are now convoluted than the men's because their ties and interests are not so clear cut. While a man clearly owes allegiance first to his own oulād, the multiplicity of a woman's kinship relations - with her own matrilineage including her brother's oulād, and with her husband's oulād - means that she has to be more circumspect than a man in making a comment about anyone. This form of debate also allows a woman to change her attitude as the discussion progresses and the points of view of other women become clear. It also makes it easier to avoid a direct confrontation between women, which can more easily occur in the style of the women's gatherings, lacking as they are in sophisticated behavioural etiquette. This does not necessarily mean that they are less structured than the men's meetings, although to an observer this might seem to be so. The complexity of their interests and the multiplicity of the ties and roles of women, require elaborate discursive formalities if successful communication is to be achieved within a system than can nevertheless pose to the 'outside', to the 'dominant model', as informal and socially unimportant.<sup>5</sup>

In their meetings the women ranked themselves, principally by one deferring to another in the order of speaking. A woman's ranking in the women's circles arose from the standing of her natal home and was sometimes affected by the position of her husband. It also took into account her own reputation and behaviour as a woman, and her ability to understand village issues, the interests of different parties, and her capacity to convey an opinion through this convoluted means of communication.

Thus it is possible for a woman to hold a higher position in the women's circles than her husband does in the male gatherings, because her personal capacities are greater. No husband would openly admit this, but others might recognize the fact. In the evening, when the husband returns home, the wife might recite a selection of village gossip to him. In the recounting of these incidents, by a series of hints and observations, a woman would put her opinion on an issue across to her husband. In the male gathering later, the husband might present a slightly different position to the one he had adopted on previous occasions. No open debate would have taken place between husband and wife, but the different stances adopted by the man in the course of a series of meetings might show that he was gradually accepting a position which accorded with that of his wife. Thus the debate which is only openly acknowledged to be between men, is also conducted in the women's circles, and the two arenas interlink in the individual households. In the household the wife can influence her husband by the opinion she has formed from contact with other women.

Moreover, the wife often has a greater range of contacts than the husband. Apart from the fact that in the agricultural seasons, the man is generally isolated on his own land during the day while his wife is in contact with other women in the village, the woman's structural position gives her greater access to different opinions. While the man is most concerned with the interests of his own oulād, she is in contact with her natal descent group (her brother's oulād and her matrilineage) and the descent groups to which her female relatives are married as well. Thus it is often the case that a woman has a wider view of the society than the husband has from the restricted perspective of his own oulād.

Individual women varied in their concern to be aware of different opinions, their personal qualities and abilities, and their capacity to influence their husbands. However, most women participated in the women's circles, and had some degree of influence over their menfolk. That this was the case can be seen from one incident when a man broke off his daughter's engagement. The women asked his wife why this had happened, and were astounded when she admitted that she did not know. The wife said that she could not ask about such an issue because she had no ekhtiār and her husband would consider her enquiries out of place and might beat her. The comments which followed this statement indicated that the other woman considered her to be very weak, and ridiculed her use of ekhtiār in this context.

Most women were able to achieve a reconciliation of their husband's and their own interests without putting the concept of ekhtiār into question. The women would not intrude into the public arena and would behave in an appropriate manner in public. No doubts could thereby arise about her husband's control of her ekhtiār. Women then operated through the women's circles and through individual contact with husbands and brothers to achieve a reconciliation of the diverse interests of their kin in the final decision - which was ostensibly made by the men. One incident was recounted in hushed tones of how, about 20 years ago, a group of women had acted overtly in political affairs. The way the incident was recited indicated that those women had shaken the accepted conventions, but at the same time the informant said that they had been justified in so acting.

They were the first and only women to inherit land. Their father was murdered when leading a faction in a fight between the two halves of the village, and left a very able wife with four daughters but no sons, and a great deal of land and gardens. His brother was also killed, so there were no males to inherit the land. The mother gave all the land jointly to the four daughters to be worked in common by their four husbands, in an attempt to keep them together in the absence of brothers. These women had therefore slightly more ekhtiār than normal, as their husbands supplemented their own income with that from their wives' land. Later, two of the husbands supported a headman who was one of their affines but also a relation of the murderer of the father. They did this in the thought that through this puppet headman they could attain the position the murdered man had tried to gain for their half of the village. The four daughters united in their opposition to this move as they did not believe the puppet headman could assert his independence from the established headman who had led the opposition against their father. When they failed to change the decision of the two husbands, the wives left them and, with the support of their sisters, went to another village until the situation was changed.

Here they were employing the most potent sanction available to them. By leaving their husbands they demonstrated that they were outside their control, and this was one way in which their husbands' ābroo was affected. By leaving, they had also removed their contribution to the domestic partnership, and while a single woman, a widow say, can maintain a household, a man cannot continue alone as he has no one to provide tea and food for his guests, and is dependent on other women even to make his bread. A man of influence, in particular, quickly loses ābroo, because he cannot invite a male gathering to his house. These four women had united to defend the name of their father and in so doing, while they had acted against most of the accepted conventions, they were still respected. They are still among the most influential women in the village, as their ownership of land puts them in a position to speak more directly to their husbands.

The four women are often seen as the fore-runners of the women who have recently become teachers. They receive a regular salary which admits the possibility of their having more ekhtiār, and they have a greater directness in their speech which they have learned from their training and their work. Many of them have married men who are teachers and are developing a new relationship with their husbands, based on a more open discussion of village affairs. One has even been given leave by her husband to speak in the male meetings at their house.

The position of both the male and the female teachers is a cause for much discussion in the village, which has only served to indicate more clearly the ideals inherent in the traditional models. The men are most concerned over the fact that the male teachers see themselves as having common interests which, on occasion, may cloud the division of society into oulāds. The teachers' more open relationship with their wives, by which they give them more right to independent action (ekhtiār) further confuses the organization of the society into oulāds, as they are verging on a situation where the boundaries of the male domain might become less distinct.

The position of the women teachers worries both men and women. While the women teachers are at pains to comply with the appropriate pattern of female behaviour, even to the extent of wearing the cotton cloak (chādor) to school, in their life-style - going to work in the morning and returning in the evening and leaving their children in the care of a female relative - they approximate more closely to a male pattern than a female one. The men's worries about the female teachers are intensified by the more open role that a few women teachers are playing in the traditionally male domain of overt discussion and decision-making. The women are equally perplexed, as the more open relationship these teachers have with their husbands, and the more direct manner of speech which they employ in conversation with other women, means that the teachers are not respecting the separateness of the women's model which was the source of their independence. This might ultimately threaten the influence women have traditionally attained through the discreteness of the women's circles and the mode of interaction with the men.

Both the men and women teachers are beginning to confuse the clarity of the structure. The two models depend on a separateness - on a demarcation of household responsibilities, on a recognition of separate domains of responsibility and activity, and on different modes of communication and behaviour. This separateness involved a dual interpretation of key concepts held in common. The concepts as employed in the dominant model excluded women from social activity, which was defined as the responsibility of the men. The obverse of these concepts and the inconsistencies inherent to their interlinkages, formed the muted model which allowed women to establish a female domain and a means of acting in society.

Beyond the separateness, the models depended on an interaction which took place in the individual households. The teachers are establishing a new form of interaction in their households. The similarity of their work and the equality of their salaries are factors which are leading these couples to reformulate the division of responsibilities between husband and wife in the domestic sphere. The greater ekhtiār which the husband then vests in his wife is clouding the separateness of the domains in the public arena. This new form of interaction between the teachers in the household has the repercussion in society that the consensus on the nature and boundaries of the two arenas is being shaken. Ultimately, both the male and female interpretations of the key concepts around which the two models focus, will need redefinition. Maybe this is why the account of the four sisters was rendered in hushed tones and a secretive manner, although the informant considered their behaviour justified: the changes are believed to be positive, but they are stretching the rhetoric, the dual interpretation of the commonly held focal concepts, to the limits of its complexity.

NOTES

1. This fieldwork was undertaken between September and November 1974, and from November 1975 to November 1976. I am grateful for financial assistance from the SSRC, Henrietta Hutton Memorial Travel Award, and the Oxford University Committee for Middle Eastern Studies, and, in the second instance, from the British Institute for Persian Studies, and the Emslie Horniman Anthropological Scholarship Fund. I should also like to thank the members of the Oxford Women's Group for their ideas and criticisms in the seminar where this paper was first given.
2. The use of dominant and muted models is after S.Ardener (1975)
3. A Darvish is a mendicant religious ascetic. It is considered reprehensible to refuse any traveller food and lodging, but this obligation is intensified in the case of a travelling Darvish.
4. 'Women's circles' is used to indicate the overlapping between the women's gatherings which produces a women's forum. This is not as institutionalized as the 'women's sub-society' described by N.S. Tapper (1968) in the Shahsevan of Azerbaijan in N.W. Iran, as there is no comparable institution to the 'xeir-u-sarr' relationships, nor any name ascribed to women leaders.
5. Here the dichotomy male/formal and female/informal roles which is used by S. Tiffany (1978) is avoided, because, as Shirley Ardener has pointed out (personal communication), this could be taken to imply that the muted model is less structured than the dominant one.

REFERENCES

- |                   |      |  |
|-------------------|------|--|
| Ardener, S. (ed.) | 1975 | <u>Perceiving Women</u><br>London: Malaby Press.   |
| Nelson, C.        | 1973 | 'Women and Power in Nomadic Societies of the Middle East' in <u>The Desert and the Sown: Nomads in the Wider Society</u> ed. C. Nelson pp. 43-59.<br>Univ. California Press. |
| Tapper, N.S.      | 1968 | <u>The Role of Women in Selected Pastoral Islamic Societies</u> . M.Phil. thesis.<br>University of London.   |
| Tiffany, S.W.     | 1978 | 'Models and the Social Anthropology of Women: A Preliminary Assessment.' in <u>Man</u> 13 pp. 34-51.   |

### TALKING ABOUT SHAMANS

'Shaman' is a term, like 'totem' or 'taboo', which has been taken from a particular ethnographic context and applied more widely. It is now part of the common parlance of anthropologists, particularly those working on societies in Asia, the Arctic, Oceania and the Americas. The shaman is considered a specialist, a person set apart by his or her ability to make controlled contact with spirits and so influence the course of events. Often shamanism depends on the possession of special instruments and on the dramatic use of trance. These seem to reflect and emphasise the distinction between shamans and laymen and, indeed, experientially, for the observer, to give the separate status of the shaman the power of a self-evident reality. Certainly, from a cursory review of the literature on shamanism - in ethnographies, in typologies of religion, and in general accounts - it is striking how many proceed by primary reference to a separable body of specialists. Research into medico-religious practices is framed by this image and normally it is expected that fieldwork will reveal concrete instances of shamans or at least traces of their earlier presence.

Logically, however, there is no reason to privilege this perspective and its widespread use is something which, in itself, requires explanation. Empirically, there are grounds to question its merits. When reading ethnographies of lowland South America one's suspicions are aroused by reports that 'approximately one out of every four Jivaro men is a shaman' (Harner 1973: 154), that among various Tukanoan groups whole sibs may be called Kumua and yet shamanic individuals within those sibs are singled out by the same term (e.g. C. Hugh-Jones 1977: 31-6), and that while 'all Piaroa men are Puwang' (Kaplan 1975: 45), Puwang is the name given to shamans and to 'the Piaroa politico-religious leader, the "man of thoughts"' (ibid).

These examples suggest that it is necessary to look more closely at indigenous language use. This does not mean merely substituting the native word for the presupposed shamanic expert or for the apparently self-evident displays of shamanic activity. Clearly it is important to know whether actions which seem shamanic or which are being performed by someone previously referred to by the indigenous term that has been glossed as 'shaman' are, in fact, spoken of in the same way by the Indians themselves. This paper attempts, through a more detailed study of the Tukanoan case, to indicate some of the problems with the traditional perspective, to outline an alternative approach, and to suggest why the traditional one has been so pervasive. To do this it is necessary to ask what is going on when we talk about shamans and, equally, what the Indians themselves are saying and doing when they use terms such as Puwang and Kumua.

#### I

Eliade defines 'classic' shamanism as controlled communication with spirits, involving the soul in an ascent to the sky or a descent to the underworld (1964: 3-8). He distinguishes shamanism from spirit possession (ibid 5-6) and in this respect is followed by de Heusch (1962). Lewis, on the other hand, argues that spirit possession is a constitutive feature of shamanism. He maintains that



To settle this issue we must go back to the main primary accounts of Arctic shamanism utilized by Eliade and also by de Heusch ... [particularly] the Arctic Tungus from whose language the word shaman derives, and whom, therefore we may take to epitomize the phenomena under discussion (1971: 50-1).

This is unsatisfactory in two respects. First, Eliade himself had written earlier that

... Tungus shamanism as it exists today cannot be considered a "classic" form of shamanism, precisely because of the predominant importance it accords to the incarnation of "spirits" and the small role played by ascent to the sky (1964: 500).

secondly, and more importantly, both Eliade and Lewis presuppose a 'true' shamanism in relation to which aberrant definitions can be dismissed or aberrant forms rationalised, while relying on specific empirical instances to establish their own 'truths'. Definitions, however, cannot be refuted empirically: apparent exceptions can always be accounted for as variations or simply as falling outside the field as specified. The argument rapidly becomes circular. An important if familiar principle is at stake: language should not be treated as a nomenclature, labelling a self-evident and given reality; nor should it be assumed that the meaning of a term is a class of empirical referents. Ostensive reference is only a provisional as well as a potentially misleading clue to meaning, a point relevant to our understanding not only of the term 'shaman' but also of indigenous terms which are treated as equivalent.

There can be no authoritative definition of either 'shaman' or 'shamanism'. To understand their meanings it is necessary to consider the circumstances of their use, both in general accounts and in particular ethnographies. Thus, for example, the different definitions suggested by Eliade, de Heusch and Lewis must be seen in the light of their attempts to provide, respectively, a contribution to the general history of religions, a formal analysis of religious experience, and a sociological account of ecstatic religion. Nonetheless, it is possible to say, following Firth, that 'The most general usage of shaman denotes a specialist in healing, divination and allied social functions' (1964: 638), though it is preferable to add that this is held to operate by means of contact with spirits rather than accept his narrower assertion that 'techniques of spirit possession and spirit control' (ibid) are employed. Similarly, 'the most significant criterion of shamanism is taken to be the state attained by the practitioner - that of dissociation, trance behaviour or "ecstasy"' (ibid: 639; emphasis original), it normally being the case that such a state is entered voluntarily and that, whatever its outward signs, it implies a degree of control on the part of the practitioner.

These definitions highlight the question of perspective. One emphasises the position of the specialist, the other emphasises an action or a state-controlled contact with spirits - which might be open to many if not all members of a society. Studies of shamanism almost invariably give primacy to the position of the shaman as a specialist, often someone observably distinct from the laymen around him. According to Eliade shamans are 'persons who stand out in their



respective societies... They are separated from the rest of the community by the intensity of their own religious experience' (1964:8), and form a 'small mystical elite' (ibid). 'The shaman begins his new, his true life by a "separation"' (ibid:13). Firth maintains that 'shamanism denotes that particular form of spirit mediumship in which a specialist (the shaman)... is deemed to exercise developed techniques of control over spirits' (1964: 638). Turner, in The International Encyclopedia of the Social Sciences, places shamanism within a classification of religious specialisations (1974), while Létraux (1949) and Jackson (1975), in the more specific context of South America, characterize shamanism solely in terms of the activities of specialist shamans. Almost all the literature on the Tukanoans in the Vaupés approaches shamanism in the same way.

## II

The Vaupés forms part of the Northwest Amazon and is a region of tropical rain forest in which population density is low. There are very roughly seven thousand Tukanoans in the area. They do not define themselves as a unit or act corporately as such, but instead are identified primarily by linguistic criteria. There are approximately twenty Tukanoan groups, among which are the Barasana, Taiwano, Desana, Bará and Cubeo. Most of these groups are recognised as exogamous and as speaking distinct 'dialects' or 'languages'. Exogamous groups are divisible into sib and both are patrilineal. The traditional settlement is a single longhouse occupied by a number of nuclear families, the men of which are normally close genealogical relatives identified with a particular sib. Women are exchanged between longhouses associated with different exogamous groups and, as a result, a longhouse always includes members of two and typically four or five different groups. Fishing, hunting, gardening and gathering are the main sources of food. Longhouse sites may change and communities may split up for a variety of reasons. There is also a great deal of travel in the area and most longhouses include a number of more or less temporary guests, adding to the mixture of different sib and exogamous group members in a longhouse at any one time. Although each exogamous group is identified loosely with a particular area and each sib, ideally, with a particular longhouse site, in fact sib members are dispersed widely in separate longhouse communities and a neighbourhood contains longhouses associated with a variety of exogamous groups. Although the Indians live off the land, they do not claim to own it. There is virtually no pressure on land and concomitantly no real sense of territory. In this apparent state of constant movement order does not derive primarily from the hierarchical and reciprocal relations between absolutely named and given groups within an observably bounded whole, nor from a universally recognised set of social offices. Instead, it is expressed in the common traditional recognition of the authority of a number of principles by which individuals and communities organise the world around them and act upon it. Relations around a naming subject, be it an individual, a longhouse community or a sib, are at the root of Tukanoan social classification and action.

Some of the earliest accounts of Indian life in the Vaupés come from Wallace and Spruce. Both travelled extensively in the Amazon around the middle of the nineteenth century, reaching the Vaupés in the

early 1850's. Both refer in some detail to the payés, pagés or shamans, of the area, their reports largely typifying the way in which hamanism was portrayed prior to intensive fieldwork. According to Wallace:

They have numerous "Pagés", a kind of priests, answering to the "medicine-men" of the North American Indians. These are believed to have great power: they cure all diseases by charms, applied by strong blowing and breathing upon the party to be cured, and by the singing of certain songs and incantations. They are also believed to have power to kill enemies, to bring or send away rain, to destroy dogs or game, to make the fish leave a river, and to afflict with various diseases. They are much consulted and believed in, and are well paid for their services. An Indian will give almost all his wealth to a pagé, when he is threatened with any real or imaginary danger (1889: 347).

In addition, it was the payé's job to propitiate the pervasive evil spirit "juruparí", or Devil (ibid: 348). Spruce creates a similar picture, mentioning in addition the special apparatus and insignia which set the payé apart. He also describes a dramatic cure at some length (1908: II: 430-44).

More recent and much more detailed descriptions are provided by Reichel-dolmatoff (1971; 1975; 1976 a, b). Indeed, shamanism is central to much of his work on the area. Initially, for the Desana, he distinguishes two types of religious specialist, the ye'e ('shaman' or 'payé' or 'jaguar') and the kumú ('priest') (1971), though this distinction is less apparent in his later works. Both, however, correspond to the traditional image of specialists as an objectively separable body of men. He mentions at some length not only their insignia of office, their conduct of dramatic cures and their receipt of payment, but also a specific and lengthy period of apprenticeship. In addition, particular emphasis is placed on the ability of the ye'e to turn into a jaguar. For Reichel-dolmatoff there is a radical and simple distinction between specialists and laymen. Referring to the Desana 'congregation of the faithful' (1971: 249), he writes:

The kumú (or kumús) are at the apex of the pyramid together with the five to eight recognized payés. These form the hierarchy. Below them are the headmen of the sik, approximately thirty, all of whom are older men. Then come one hundred and fifty heads of nuclear families... The young initiated, approximately sixty, form the base. These five categories, a total of some two hundred and fifty individuals, form the active and participating congregation (ibid).

Other research, based on lengthy fieldwork, seems to confirm this picture in several respects. Goldman (1963), for the Cubeo, and Stephen Hugh-Jones (1974), for the Parasana, for example, refer to a specific period of apprenticeship and Goldman continues to give some emphasis to curing rituals. He distinguishes two types of shaman, the pariékoku (man of power) and the yaví (jaguar), the latter being not only more

powerful but also more capable of inflicting harm by turning into a jaguar (1963: 262-70). Stephen Hugh-Jones, and also Langdon, refer to an ordinary shaman, kumu, but also note that the most powerful shamans are 'sometimes referred to as yai, jaguar' (S. Hugh-Jones 1974: 103) or that on occasions shamans are considered capable of turning into jaguars (Langdon 1975: 161). Men described by these various terms seem to stand apart from other Indians, as objects of fear or respect.

In other ways, however, field research suggests that the situation is more fluid than is implied in the writings of Reichel-dolmatoff and the earlier travellers, and prompts a re-examination of their evidence and arguments. It may be argued, of course, that variations in evidence are solely a function of local variables such as exogamous group practice, environment or degree of 'white' influence. Certainly, it is necessary to avoid too syncretistic an approach and these variables are not without significance, but there remain grounds for suggesting that a particular frame of reference has influenced the collection and presentation of data. Torres Laborde's account of Barasana mythology and culture (1971) corresponds very much more closely to Reichel-dolmatoff's approach than to that of Langdon or the Hugh-Jones; Reichel-dolmatoff himself is willing to generalise many of his conclusions for the area as a whole; and, indeed, when the information collected from the traditional perspective is examined more closely, purely internal inconsistencies and contradictions are revealed.

The references to an enabling apprenticeship and to the acquisition of insignia of office may be taken as an example. These two issues, in fact, largely reduce to one, for it is during the period of training that the novice is held to gain many of the ceremonial objects, as well as knowledge of myths, charts and genealogies, which subsequently imbue him with shamanic power and provide ostensive markers of his position. Both the apprenticeship and the insignia seem to endorse the objective separability of shamans. On closer inspection, however, there are several reasons for reconsidering their significance.

Reichel-Dolmatoff maintains that the apprenticeship for a shaman takes place over a period ranging from a few months to a year and that 'generally, the apprentice goes to another phratry or tribe [exogamous group] to be instructed there at the side of a famous payé' (1971: 127). yet almost all the aspects of oral tradition - the myths, chants and genealogies - are held to be the exclusive property of either particular sibs or particular exogamous groups. In such a situation it would be impossible for an outsider, a member of another exogamous group, to pass them on. There seems to be some misunderstanding of what the Indians are saying.

Secondly, it is not clear that the alleged period of apprenticeship is radically distinct from other learning sessions which might be repeated often during an individual's life. Reichel-dolmatoff himself records periodic gatherings in which 'a group of men, perhaps three or four of them, will ask a payé to teach them how to cure certain diseases or how to harm their enemies' (1975: 96). The description of these sessions is very similar to accounts of both male initiation and the shaman's apprenticeship and, indeed, Reichel-dolmatoff on occasion refers to the participants as 'apprentices' and to the sessions as 'shamanistic reunions'. Although a number of other sources refer to a special period

of apprenticeship, Tangdon mentions that no such event was described to him and that the requisite knowledge apparently was passed on by the more learned members of a group over a prolonged period (1975: 198). The gradual acquisition of knowledge, power and interpretative skills seems more important than any single period of training.

This is endorsed by a third consideration, the fact that many of the things said to be obtained during the apprenticeship can be, and are, acquired in other ways and, concomitantly, are open in principle to all adult men and in practice to many of them. According to Stephen Hugh-Jones, shamanic skill is based on the ability to acquire, remember and clearly interpret as many myths and chants as possible. Many of these aspects of a group's tradition are passed on during male initiation and in more informal contexts before and after this by the older and more knowledgeable members of the group. Reichel-Dolmatoff himself suggests that one of the personal qualities which a man should demonstrate from childhood onwards in order to be considered capable of acting as a shaman is 'a profound interest in the religious traditions of his culture' (1975: 127) and 'a good knowledge of myths, genealogies and invocations' (ibid). The capacity to contact the spirits in a controlled way, through the use of drugs, is also developed gradually from initiation onwards by all adult men, under the supervision of skilled practitioners in both private drug-taking sessions and on ceremonial occasions (e.g. S. Hugh-Jones 1974: 119). Similarly, many of the ceremonial and ritual implements and decorations said to be obtained during shamanic initiation can be obtained elsewhere, by other means. Some are passed down through an individual's immediate patriline, generally on the death of their previous user. The individual thus acts conditionally as their user or bearer, rather than as their owner, and does not necessarily acquire them at a time coincident with any supposed period of training. In the same way, certain decorations are handed out by the local headman, the 'father' of the settlement, at ceremonies, different decorations marking off different roles for the duration of the occasion. These articles can also be obtained by trade, by manufacture (under supervision) and in the past, it is said, by warfare, one of the primary aims of raiding being the capture of other peoples' ornaments. The possession of these various objects is recognised as a marker of power and prestige, but the capacity to obtain them is not restricted to a specific period of apprenticeship. An ambitious and well-placed individual can acquire them in a number of ways. Nor is it clear that specific insignia are universally recognised as distinguishing a shaman from other men of power. Goldman notes, for the Cubeo, that the distribution of objects traditionally associated with shamans, such as girdles of jaguar teeth and armadillo vestments, is neither uniform between sibs nor intrinsic to the status of shaman. They are 'symbols of shamanistic power' (1963: 154) but 'they are not shamanistic insignia' (ibid) and the same may be said of the quartz cylinders found in the area. Several of the insignia and objects of power, for example the splinters and crystals implanted in the apprentice's body, are considered invisible for long periods of time. Others, such as the skins of jaguars and anacondas which are held to mark off shamans and to enable them to disguise themselves as these animals, sound more substantial and visible. As Reichel-Dolmatoff notes, however, the word for 'skin' is used also in a broader and more abstract manner to describe a state or mood. He writes:

when speaking of the jaguar skin or garment our informants employed the term surirō. Although this is the common word used to designate any garment or attire, be it of cloth or bark, it is also used to describe a particular state, in the sense of a person's being invested with, that is clad in certain qualities. The elder informants - and we have no reason to doubt their words - insisted that it was in this sense that the transformation had to be understood. In fact, it became clear that many emotional attitudes could be described by this term, it being understood that on these occasions the person was imagined as being covered by a kind of invisible envelope expressing his mood or state (1971: 125).

Thus Indian statements which apparently refer to objective entities can, in fact, refer to qualities or transient states open to the subjective assessment of observers.

Knowledge and power are represented often as embodied in or consequent on the possession of objects or substances available in principle to all adult men. Although Goldman points to a special period of apprenticeship for the pariékoki (ibid: 264-5), he also stresses the importance of the concept of power in relationships not only between the two types of shaman, but also between the shamans and the so-called laymen. All Cubeo value and see this power and, in various ways and to varying degrees, are able to accumulate it over time.

The native term for power is pariē, a term that applies to the strength and vigour of a warrior, the magical potency of the ancestral flutes and trumpets, the awesome fierceness of the jaguar, as well as the clearly magical powers of the shaman or of the laymen who has learned how to cure and to practice sorcery (ibid 262).

Finally, there is no evidence that even if an individual undergoes some more esoteric form of instruction than his peers that this, in itself, distinguishes him radically from those around him or that it enables him immediately to act as a shaman. According to Reichel-dolmatoff, training takes place at around the age of twenty five (1971: 127), yet references to shamans almost invariably emphasise their seniority and experience.

These various factors call into question the existence of a single, enabling period of apprenticeship and a concomitant set of objective markers of status. The gradual acquisition of knowledge, power and interpretative skill seems more important than any such initiation and further undermine the picture of a radical and simple distinction between specialist shamans and laymen. Behind the appearance of a separable body of shamans lie differences of degree between adult men.

In what ways do these differences of degree become concretised terminologically by the ascription of terms such as pariékoki, kumū, or yeie? Subjective factors seem critical. The statement 'he is a shaman', or 'he is a jaguar', seems to rely to a large extent on personal or consensual appreciation of a particular state (or 'skin') which, perhaps only temporarily, another person, or other people, are held to fill.

Lévi-Strauss has pointed out that a vital element in the 'shamanistic complex' is the social consensus as to the powers of the shaman, though he makes his argument unnecessarily narrow by confining it to the instance of curing and to the role of the group as participant in the cure. He observes that

the fundamental problem revolves around the relationship between the individual and the group, or, more accurately, the relationship between a specific category of individuals and specific expectations of the group (1963: 180).

He sums this up, aphoristically, by observing that Quesalid did not become a great shaman because he cured his patients; he cured his patients because, in the eyes of the group he was a great shaman (ibid). It is not sufficient either to undergo the qualifying experiences, or to possess knowledge; it is necessary to have these factors recognised.

In the Tukanóan case what is being recognised is a degree of skill considered sufficient to enable a particular individual to occupy a specific position. Certainly, the Barasana evidence suggests that the position of shaman may be a role necessary for certain ritual occasions. The position will be filled by the person considered most competent to do so: people recognised as shamans may occupy as well the role of headman; at male initiation 'if there is a shaman in the house, he will be asked to officiate at the dance, otherwise a guest from another house must be asked to fulfil this role. Sometimes the host himself acts as either shaman or lead-dancer' (S. Hugh-Jones 1974: 22-3). However, the position of shaman is not equivalent to an 'office' because it does not operate in a single context, at a single level of extension. Different ceremonies, from the domestic cure to the inter-longhouse gathering, have different socio-geographical ranges and each context entails a shamanic position. The situation is best summarised by Christine Hugh-Jones:

At present, possession of one ritual role does not exclude an individual from possessing another and for any specialist role there are degrees of competence and recognition. Within a large longhouse there is usually at least one individual with some degree of competence in each of the roles of chief, dancer/chanter and shaman... In a set of longhouse groups that meet regularly for ritual purposes, there will be at least one important ritual specialist shaman, one chanter and one dancer of wide renown; thus the distribution of the ritual roles depends largely on the fact that they are indispensable for the functioning of a ritual community... The general lack of consistent role identification for individuals within the social structure together with the need to fill the roles results in a system of specialisation resting more upon relative personal aptitudes and ambitions than social structural status (1977: 34).

Terms such as kumú and ye'e thus seem to describe positions, states or 'skins' rather than a separable group of people. This image is reinforced by a closer examination of other ways in which they are used. kumú, or the cognate form gumú, refers to the main longitudinal beam of the longhouse, to the long tongues of rock which are scattered through



the forest, and to a bench or stool (Reichel-Dolmatoff 1971: 106, 135-6; Jangdon 1975: 68). Ye'e, or yai, refers to the central houseposts, while the term for houseposts is also used for the steep pinnacles of rock in the area, and for star (Reichel-Dolmatoff 1971: 106; Jangdon 1975: 68, 75). It also, of course, refers to the jaguar and, more generally, to predators including anacondas and eagles (e.g. Reichel-Dolmatoff 1971: 129; S. Hugh-Jones 1974: 138). Three points are important here. First, the terms are used to describe constitutive features of the structure of the longhouse and of the earth, necessary parts of a given whole. Secondly, the same terms refer to structured wholes on radically different scales. Thirdly, the rocks or mountains are described frequently as the longhouses of the ancestors or of the animals and their spirit guardians, and, more generally, all the things covered by these terms are spoken of as places where individuals make contact with spirits. These various points challenge the assumption that words such as kumú and ye'e identify a separable group of specialists and demand a reconsideration of suggestions that the shaman (ye'e) turns into a jaguar (ye'e). The relationship between the people and the positions described by the words is contingent in two senses: the states may be experienced only transiently, the skins occupied only temporarily; and the positions themselves may cover a wide range of socio-geographical referents. Undoubtedly, from a particular point of view, everything will appear as if there were a group of people objectively qualified and widely recognised as shamans, but it seems more advantageous to approach this situation from a different perspective. Just as it is wrong to assume that the 'true' meaning of 'shamanism' lies in an empirically separable set of Tungus specialists, so it is wrong to assume that the 'true' or primary meaning of these terms is a separable set of Tukanoan specialists and that their use in other contexts is merely derivative or metaphorical.

kumú and ye'e describe positions, but more than this their use implies the creation or recognition of a relationship between people occupying different positions in a given context. Thus not only can an individual be described as occupying the position of kumú in relation to a particular individual or community, but whole sib can be described by members of another sib as 'our shamans' or as shamans in relation to a third group. In all this the position of the naming subject is crucial. This does not apply only to the subjective assessment of degrees of skill or transient states. In a purely relational system of naming there are no absolute terms. Only from a particular point of view, that of the naming subject, does there appear to be an intrinsic identity between the name and the object so described. Kinship terminologies exemplify this. It seems useful, in the Tukanoan context, to regard statements such as 'he is a shaman' or 'he is a jaguar' as analogous to statements such as 'he is a brother' or 'he is a cross-cousin'. In such a situation it is impossible, taking the Tukanoans as a whole, or indeed any locality in the Vaupés, to count the number of shamans. Indeed, it is as meaningless as proposing to count the number of cross-cousins in a society. Counting can only take place from a particular point of view, but even here the association between categories and people is ultimately contingent. The Tukanoans themselves recognise that, in the last resort, terms such as 'cross-cousin' and 'brother' describe positions rather than people. Particular groups that were once cross-cousins can become brothers and vice-versa. In the Vaupés at least, it may be more advantageous to approach shamanism from the point of view of a naming

subject occupying a given position in a system of relations than from that of an apparently separable body of specialists. This is not to challenge a supposed 'reality' by appeal to a relativism which merely takes at face value the 'ideology' of the people being studied. It is to recognise that what the Tukanoans say is central to how shamanism works for them, and to challenge the simple formula that counts as 'true' what we observe while ascribing to 'ideology' those indigenous statements that fail to match this 'truth'.

On the face of it, however, there remains a solid body of evidence which confirms the traditional image. There are frequent references to shamans as professional specialists acting on behalf of unqualified laymen and, in the early accounts at least, to dramatic cures in which the separability of the shamans seemed confirmed. Yet it is striking how little has, in fact, been observed. Spruce, the nineteenth century traveller, provides a candid but startling admission:

I have never been so fortunate as to see a genuine payé at work. Among the civilised Indians the Christian padre has supplanted the pagan payé, who has besides been discountenanced and persecuted by the civil authorities; so that if any now exist, he must exercise his office in secret. With the native and still unchristianised tribes I have for the most part held only passing intercourse during some of my voyages. Once I lived for seven months at a time among them, on the river Uaupés, but even there I failed to catch a payé (1908: II: 431).

Despite this failure, he still felt able to provide a detailed account of the activities of the payés in the area, including a lengthy description of a cure. Wallace similarly provided a thorough but unwitnessed description. Reichel-dolmatoff's accounts, once again including detailed descriptions of cures, are based on only limited visits to the field. In particular, Amazonian Cosmos (1971) derives almost exclusively from evidence provided in Bogotá by a single, 'acculturated' pesana informant. Goldman saw only one 'full' cure. He also admits that he could never clarify the relationship between the yaví and the jaguar because no yaví was willing to talk about it.

Despite these problems the traditional image has continued to frame people's expectations and influence their research. Jackson arrived with the intention of studying 'the beliefs and practices related to disease and curing' (1972: 1) and of working with 'the disease specialists of the area' (ibid: 2), but eventually changed her project. Langdon had a similar aim:

When I began my fieldwork I wanted to do a thorough study of an indigenous medical system, concentrating on curing rituals as a form of psychotherapy. I had hoped to follow illness cases and analyse the therapeutic process in terms of the shaman's use of symbols and his manipulation of social relations (1975: 1).

He found the situation more fluid than he had imagined and had difficulty in extracting the required information from the respected curers. In

particular, he discovered that dramatic curing rituals were an exceptional rather than a typical part of the shaman's activities. Most curing took the form of the private and inconspicuous blessing of foodstuffs in order to make them safe to eat, a skill which was accessible to all adult men and practised by many of them to a greater or lesser degree.

There is thus very little eye-witness evidence of dramatic cures and certainly no basis for considering them a typical marker of the separate status of the shaman. There is even less direct evidence of apprenticeships. Although a number of fieldworkers have witnessed male and female initiation ceremonies, none have seen novices undergoing a specific training in shamanism. Goldman admits that 'none of my laymen informants could describe it in full' (1963: 264) while no-one else would (or could).

Frequently, writers have sought, as Spruce did, to explain these absences in terms of either the influence of 'whites' or the necessary secrecy surrounding shamanic activity. The lack of the anticipated numbers and type of shamans has been used commonly as an index of cultural collapse in the face of 'white' repression. Without denying that there may well have been instances in which 'whites' persecuted those they had labelled as shamans, it may well be that these arguments about secrecy and decline are elaborations attempting to make good a false set of presuppositions. The silence of certain informants may derive from the fact that the questions they were asked did not make sense to them.

### III

If, in the Tukanoan case, there are grounds to doubt the accuracy of a picture of readily separable specialists and, more generally, to question the merits of an approach which begins from this point of view, it remains to offer some suggestions as to why this perspective has been so pervasive in the literature on the Tukanoans and on shamanism as a whole.

In the Vaupés a great deal of the evidence has come from what people have said rather than from what they have been observed to do. It thus becomes particularly important to examine the assumptions that underlie the way in which language is used by 'white' informants, by anthropologists, and by the Indians themselves. Many of the early writers were uncritical in this respect. They relied heavily on what local traders told them and on what earlier travellers had written, seeming to accept the existence of a stereotypical mystical specialist throughout the Amazon without considering the specificity of earlier accounts or the extent to which they were part of a tradition informing later sources. Spruce, for example, felt that the similarities between local accounts of payés and seventeenth and eighteenth century descriptions of shamanism in the Caribbean and Guayana were so great as to allow him merely to quote the latter rather than elaborate on the former (1908: II: 432-4).

This assumption of a stereotypical specialist was encouraged by the widespread use of the term 'payé'. It is derived from words found in both Tupi and Carib languages and, in its Tupi form, became absorbed into nheenghatu or língua Geral, a pidginized version of Tupi developed

by the early jesuit missionaries in Brazil and used by traders and explorers throughout the Amazon as a means of communication with Indians. The term encompassed the idea of a medicine-man, a medico-religious specialist capable of contacting spirits in a trance in order to influence events. Its range of reference was thus very similar to that of 'shaman' and the two are now treated as synonymous. Reichel-dolmatoff, for example, uses the terms interchangeably. Furthermore, he treats indigenous terms such as *ye'e* as merely local versions of the ubiquitous form 'payé', referring for example to 'The native word for payé ...' (1975: 101). The Tukanoans are not members of the Tupi language group, however, and 'payé' is not an indigenous term. Although it gained some currency at the turn of the century with the influx of rubber dealers it is rarely if ever used in conversation between Indians (Sorensen 1967: 680). A number of *lingua geral* terms are used in conversation with 'whites', but it may be that, where matters of religious significance arise, these terms are used merely to obfuscate in the face of intrusive 'white' interest. Such is apparently the case with the word 'Jurupari', which is widely used in the early literature to refer to the devil, devilry and, by association, much Indian religious activity (Goldman 1963: 192; S. Hugh-jones 1974: 6-7). The same may be true of 'payé', a term used to demarcate the specialist often said to deal with 'Jurupari'.

Thus, it is not clear that a great deal of authority can be vested in the early accounts or that the situation they describe can be taken as a basis for a measure of subsequent change. More recent accounts of the Vaupés, and more general works on shamanism continue to approach the problem in a similar way, however. There seem to be a number of reasons for this beyond their mutually reinforcing effects.

First, there is a particular view of language which treats it as a nomenclature and assumes that the meaning of a term, particularly one referring to people, is an ostensibly specifiable class of phenomena. This has little credibility as a general theory of meaning but seems to be encouraged in anthropology by the methodological necessity in field-work of giving priority to the collection of ostensive definitions.

Secondly, and more specifically, the assumption of an intrinsic identity between a person and his occupation is common in our own society and appears to be extended to others. Normally, we introduce people according to their occupation, particularly if they are 'members of the professions', and, concomitantly, we make a number of assumptions about the nature of the person's skills and qualifications. For example, if we say 'he is a doctor', we assume that the person so described has undergone a period of training and that, if necessary, he could show us the insignia of his position - a doctor's bag, a stethoscope, or, more securely, a set of certificates. It is the successful training that entitles the man (or woman) to act as a doctor in the eyes of every member of a given society. We assume, further, that given this clearly separated status, we could count the number of doctors. It seems that, in the study of religious specialists, indigenous terms have often been treated as being used in a way analogous to our own use of terms such as 'doctor' and that this usage creates an idiom which significantly influences our articulation of indigenous systems. This is not to suggest that there is an absolute difference between the way we talk about people and the way that the Tukanoans, for example, do. A term

such as 'singer' seems, in all but the relational sense, to approximate more closely to the word kumú, covering certain animals as well as people, describing actions, states, ceremonial roles or widely recognised abilities, and applying in one instance to almost everyone and in another to only a few. Nonetheless, the image of the doctor seems widespread in the study of medico-religious specialisation. References to shamans as 'part-time' specialists merely confirm the primacy of the model of professional occupation. This model carries with it expectations about status and qualification. In the Tukanoan case at least, it seems possible that it has generated descriptions of a distinct apprenticeship that does not really exist in this way.

A third factor concerns the relationship between the status of the subject and concomitant images of the social. It is common now, in the Vaupés and elsewhere, to conduct fieldwork by intensive study in a single settlement over a period of time which is necessarily short in relation to significant changes for that settlement. From such a point of view, particularly in societies that lack any formal institutions, things may seem more fixed than they are in reality. The position of the naming subject is something that should not merely be taken for granted and generalised to characterise a society as a whole.

#### IV

The primary aim of this paper has been to question the merits of a particular way of approaching medico-religious practices both in general terms and by reference to a specific ethnographic context. Although the paper has concentrated largely on the latter, there may be grounds for re-examining the study of mystical specialists throughout lowland South America and perhaps more widely. Significantly, Alan Campbell has come to conclusions concerning the Tupi-speaking Oyampi similar to those suggested here concerning the Tukanoans. He argues that the indigenous term i p. ye is much better seen as an adjective, indicating a quality, than as a substantive indicating a role or office. He does not refer to the relational uses of such a term, but argues nonetheless for a change of perspective in which the office is approached as something constituted by the quality in the way that the office of village strong-man is constituted by the quality of strength (personal communication).

This paper suggests that there are grounds for a closer attention to the position of the subject. This applies not only to the specific instance of Tukanoan shamanism, but also more generally. The Durkheimian exorcism of the individual has meant that the status of human agency has been problematic for both functionalist and structuralist analyses. Concomitantly, insufficient attention has been paid to the capacity of social actors to produce meaning in the use of language. The position of the subject should not be taken for granted but should be explained and, in turn, become explanatory (cf. e.g. Coward and Ellis 1977).

In a related way it may be beneficial to look at the relational aspects of terminologies in areas other than 'kinship'. Nick Allen, in an as yet unpublished paper on the logically simplest forms of social structure (Allen n.d.), argues that purely relational systems are logically prior to ones involving absolute designations and that

functional specialisations associated with these systems should be seen in the same light. certainly, in the Vaupés, there seems to be some advantage in proceeding from the assumption of relative designation.

Allen also suggests, as a hypothesis, that relational systems are chronologically prior. He does not consider in any detail the conditions of a move towards an absolute system, but it is possible that one of these might be the creation of a referential identity between a group and a particular territory as happens when nomadic hunters and gatherers gradually settle and come into conflict with others over land. A more precipitate basis might be the influence of 'white' or 'European' contact. The 'creation' of tribes and chiefs is a familiar motif both in the evolution of agricultural communities and in histories of colonialism and cultural imperialism. In the Vaupés these trends are readily identifiable. They are both prefaced and reflected by the way in which the Indians are described. It may well be that as a part of this process the 'whites' created the shaman, at least in the form of a separable specialist. The traditional shaman may exist only in the fragments of language and spruce may have failed to catch a payé because he was hunting a mythical beast.

Roger Rouse

#### NOTE

I should like to thank Christine Hugh-Jones for her comments on an earlier draft of this paper. The mistakes in the present version are, of course, mine alone.

#### REFERENCES

- |                             |      |   |
|-----------------------------|------|---|
| Allen, N.J.                 | n.d. | 'Tetradic society: An Essay on kinship and social structure'. Unpublished manuscript.   |
| Coward, R.<br>and Ellis, J. | 1977 | <u>Language and Materialism</u> . London: Routledge & Kegan Paul.   |
| Eliade, M.                  | 1964 | <u>Shamanism</u> . Trans. William R. Trask. New York: Pantheon Books.   |
| Firth, R.W.                 | 1964 | 'Shaman'. In J. Gould & W.L. Kolb (Eds.), <u>A Dictionary of the Social Sciences</u> . pp. 638-9. London: Tavistock Publications. |
| Goldman, I.                 | 1963 | <u>The Cubeo</u> . Urbana: The University of Illinois Press.  |



- Harner, M.J. 1973 The Jívaro. London: Robert Hale & Co.
- de Heusch, L. 1962 Cultes de possession et religions initiatiques de Salut en Afrique. Annales du Centre d'Etudes des Religions. Brussels.
- Hugh-Jones, C. 1977 Social Classification Among the South American Indians of the Vaupés Region of Colombia. Unpublished Ph.D. thesis. University of Cambridge.
- Hugh-jones, S. 1974 Male initiation and Cosmology Among the Barasana Indians of the Vaupés Area of Colombia. Unpublished Ph.D. thesis. University of Cambridge.
- Jackson, J.E. 1972 marriage and Linguistic Identity Among the pará Indians of the Vaupés, Colombia. unpublished Ph.D. thesis. Stanford University.
- - 1975 The Ethnography of Lowland South America. In B.J. Siegel (Ed.), Annual Review of Anthropology. Vol.4. Palo Alto: Annual Reviews.
- Kaplan, J.O. 1975 The Piaroa. Oxford: Oxford University Press.
- Langdon, T. 1975 Food Restrictions in the Medical System of the Barasana and Taiwano Indians of the Colombian Northwest Amazon. Unpublished Ph.D. thesis. Tulane University.
- Lévi-Strauss, C. 1963 Structural Anthropology. New York: Basic Books.
- Lewis, I.M. 1971 Ecstatic Religion. penguin Books.
- Métraux, A. 1949 Religion and Shamanism. In J. Steward (Ed.), Handbook of South American Indians. Vol.5. pp. 559-599, Washington: United States Government printing Office.
- Reichel-Dolmatoff, G. 1971 Amazonian Cosmos. Chicago: The university of Chicago press.
- - 1975 The shaman and the jaguar. philadelphia: Temple university press.
- - 1976a cosmology as Ecological Analysis. man. (NS) 1: 3. pp. 307-318.
- - 1976b Desana curing Spells. J.L.A.L. 2:2 pp. 157-219.
- Sorensen, A.P. 1967 Multilingualism in the Northwest Amazon. American Anthropologist. 69:6. pp. 670-684.

- Spruce, R. 1908 Notes of a Botanist on the Amazon and Andes.  
2 vols. London: Macmillan.
- Torres Laborde, A. 1971 Mito y Cultura entre los Barasana. Bogota:  
University of the Andes.
- Turner, V.W. 1974 Religious specialists. In D.L. Sills (Ed.)  
The International Encyclopedia of the Social  
Sciences. Vol. 13. pp. 437-443. Macmillan  
& the Free Press.
- Wallace, A.R. 1889 A Narrative of Travels on the Amazon and Rio  
Negro. London: Ward, Lock & Co.

## GRASSHOPPERS AND SLUGS

Fearing myself stuck at the pre-1961 stage of butterfly collecting in social anthropology (see Needham 1978), I decided to take myself in hand and so read with consuming interest some of the more esoteric passages in the latest number of JASO, starting with the piece on Godelier by Jan Ovesen. Here I'll try and say what I've gleaned and how it all seems to a poor tired mind long since gone to biological seed. A futile exercise, perhaps; it's boring, I know, to have to listen to biologists with their tiresome clichés about natural selection, but I'll do my best to keep your interest.

Well, 'kinship' it seems, is an empty category. Needham, I think, told us this at an ASA conference in Bristol many years ago. So it goes without saying that 'matrilineage' is empty too (Leach 1961, Needham 1974) and I should have known better. Likewise with 'incest', as I discovered personally in October 1976 — two years after Needham had demolished the concept — when I somewhat rashly attempted to give a paper on the subject (albeit in monkeys) at the Oxford Institute of Social Anthropology. Now Ovesen reviewing Godelier and using this as a platform to launch a critique of the Marxist concepts of infrastructure and superstructure, concludes after what I found to be a very thoughtful analysis, that 'it seems to me that the net result is to make nonsense out of the notions of infrastructure and superstructure' (1978:10).

In place of a simple duality: infrastructure -- superstructure, Ovesen sees Godelier as wanting to have a sort of equilateral triangle of infrastructure, superstructure and ideology with one of these in each corner and arrows between each corner and the others. But Ovesen disagrees and wants to jettison the whole scheme in favour of a far more basic reversion to categories and category systems as the final units of analysis for nature (infrastructure), society (superstructure), and ideology (something to do with power).

The same idea is reiterated by Malcolm Chapman in his piece in JASO. Discussing the debate about 'structuralism' he joins others to criticise it for theory-building: not just those who from a marxist viewpoint see it as failing to put first things first, but also those who attack all efforts to discover 'fundamental' principles in human social life, including fundamental characteristics of the human mind. Together with Crick he prefers to see categories as more free floating altogether — bits of ice forming a pack, they can get bigger or smaller, get shoved out or grow, but at any time they more or less fit together. Levi-Strauss thus gets a hammering. So does Loizos, whose 'normative' analysis, is made to sound positively dinosaurian ('Am I alone, I wonder', writes Chapman 'in finding in the word "norm" a drug to make my heart sink?' (1978:39)). There are no fundamentals, nor even cross-cultural regularities, just the arbitrariness of Saussurean signs, the 'loom of language' as Frederick Bodmer once put it, carving up the (to us) ever-hidden 'real world' and presenting us with the pieces to juggle with as best we can. With the return to language as the arena of debate we are firmly on Edwin Ardener's terrain, 'kneedeep in polarities' of our own kind (not theirs, whoever they are), where cross-cultural translation is inevitably imperfect and in a strict sense impossible (he told us so at the Oxford ASA meeting some years ago); much of Chapman's thinking clearly derives from Ardener (1973, 1975). Peeping below or behind language Ardener finds a 'sluggishly moving continuum of social perceptions' and that's as close to a 'fundamental' process as I can find anywhere in the debate.

Some, (and here I come on to the piece by Maryon McDonald) clearly don't want to go peeping below, they just want to wander among the scintillating flowers of the word-garden. And why not? Why be 'ernsthaft'? No-one forces us to be. Marvin Harris gets a pasting for being a stickler for empiricism, for holding out for science. Why should scientific categories be holier than others? OK, OK, McDonald clearly has great fun with words, as my hero Jean-Paul Sartre always did (and we don't need to look at the later Sartre, standing, as I cannot forget seeing him in a newspaper photo, on an upturned dustbin outside the Renault factory near Paris like a Jesus-freak with the workers flowing past him to the near-revolution of '68).

I was thinking of all this, and the radio was playing Steve Hilage and Genesis and things like that, and time was flowing over the edge of category-April-11 into category-April-12, and I fell to wondering why I gave up social anthropology in 1959 and went to the freezing cold Dunstable Downs to watch rhesus monkeys. At the time I told my supervisor Michael Chance it was because I found humans too complicated and hoped monkeys would be simpler. Today I find my subsequent discoveries the subject of a demonstration by Edwin Ardener of how the human mind thinks, and I agree with him.

I agree with Malcolm Chapman, too, about his 'shimmering surface of a poetics' (derived from Thom) and was surprised and gratified to learn that Evans-Pritchard thought he'd have done his job better as a poet. We humans have arrived in a new world, quite different from the ones our pre-human ancestors inhabited, and the ones our contemporaries in other species inhabit now. They, now, can and do exist purely: we live contorted, tainted lives.

Perhaps then it was the quest for purity, for a more real reality, that led to the monkey? I don't know. But I have two questions for the social anthropologists, and here I will stop. First, are you content to give up on 'reality'? If not, I suggest you'll have to turn to developmental psychology and (groans) biology. You can ask me why I suggest this if you want, but I'd like your views first on my question. Second, how do you imagine that your World III, your ephemeral chimera of the signified, relates to the atomic particles of which all physical matter is constructed; and if it doesn't, what's it made of? This one really intrigues and puzzles me.

Vernon Reynolds

#### REFERENCES

- |              |      |  |
|--------------|------|--|
| Ardener, E.  | 1973 | <u>Some Outstanding Problems in the Analysis of Events.</u> ASA Conference Paper.                              |
| Ardener, E.  | 1975 | <u>The Voice of Prophecy. Further Problems in the Analysis of Events.</u> The Munro Lecture, Edinburgh: unpub. |
| Chapman, M.  | 1978 | 'Reality and Representation', <u>JASO</u> Vol. ix, no. 1, pp. 35-52.   |
| Leach, E.    | 1961 | <u>Rethinking Anthropology</u> , London: Athlone Press.  |
| McDonald, M. | 1978 | 'Language "At Home" to Educated Radicalism', <u>JASO</u> Vol. ix, no. 1, pp. 13-34.                            |
| Needham, R.  | 1974 | <u>Remarks and Inventions</u> , London: Tavistock.   |
|              | 1978 | Letter in <u>T.L.S.</u> , April 7th, p. 393.   |
| Ovesen, J.   | 1978 | ' Maurice Godelier and the Study of Ideology', <u>JASO</u> Vol. ix, no. 1, pp. 1-12.                           |

## THE MISSING LINK

Vernon Reynolds asks the social anthropologist: 'Are you content to give up on "reality"? ... how do you imagine that your World III, your ephemeral chimera of the signified, relates to the atomic particles of which all physical matter is constructed...?' (Reynolds 1978:130). It is a well-established enthusiasm - and one that still ostensibly guides research programmes around the world - that attempts to build the sciences one into another, from the micro- to the macro-, in a hierarchy of chinese boxes where each one is fully explicable in terms of all the others, such that the only distance between sub-atomic particles and development economics is one that research can be expected to fill, given time. Do we still cherish that ambition in social anthropology, or, indeed, in human biology? We have certainly discovered, as Reynolds observes, that the simple institutional unities of our academic discourse have, perhaps as the necessity of their existence, the capacity to create their own orders of significant reality, hermetic to the intrusive intellectual from down the road. At the same time, we have found that applying our accustomed 'realities' of economics, politics and law to alien societies has shaken ready guarantees and collapsed the easy security quietly offered by common-sense. But this collapse has not required that such 'categories' be jettisoned as unreal, any more than it has opened a path to 'reality', or to a final referent on and around which a whole and integrated edifice of man could henceforth be neatly erected.

We have, perhaps, admitted reluctantly that all assumptions of definite judgement involve an a priori claim to clear sight, and that to interrogate that claim as to the source of its security is to embark on the chase for some ever-unstated or ineffable warranty, to risk 'infinite regression' and 'circularity'. Nevertheless the claim to security continues to be made with many idioms for its expression. And it seems that the safest, the most irrefutable, the one least likely to collapse beneath the blow of a stout Johnsonian fist, is materialist reality (which we will spare, for the moment, the cosmetic embarrassment of its quotation marks). The problem has, of course, a long and amply - (if not well) - documented history, and it is no dissolution of the problem merely to point this out. However, if we are to be asked how the real (the atomic particle) relates to the idea, and if we are advised to fill the evident gap with biology, then, persuasive as this might seem, there are certain rather common-sense and smaller-scale queries that we are entitled to make. Now, for example, does the atomic particle relate to the chemistry of the protian? How does the biochemistry of DNA relate to the concept of the gene as popularly it is understood in evolutionary theory? How does cell chemistry relate to brain function? These might all seem to be answerable questions, and until they are answered the student of the social can be forgiven for not bothering himself with elementary particles. Of a slightly different order, and perhaps more pertinent here, are questions like the following: how does neurology relate to social psychology? how does psychology relate to psychiatry? how does behavioural genetics relate to primate ethology? The answer 'not very well' means that the social anthropologist, however keen he is to find the solid empirical grounding for his study, has no clear path to it through the various disciplines concerned with man that all consider themselves, in their amiably different ways, 'hard'.

There can be little doubt that man is living in a real world - and yet it would be no surprise if, say, freezing out on the Downs,

and getting on with the job, should feel more real than musing through the night with music and memories. It's that arbitrariness again, putting the ground under our feet and ideas in our heads, and still sending us off in pursuit of the real. Man's ambitions and ephemeral significations always are inscribed, and not just as simple 'bias', or 'prejudice', whether he sits alone amidst baboons, or among an international scientific elite at the centre of a cyclotron, or in uneasy co-existence with an alien people. And to say this is not to deny the potency of scientific rationality, or to render fictional the achievements of modern medicine, or to deny the efficacy of atomic bombs. Moreover, it is emphatically not a retreat into the word garden, into the clutches of an ephemeral chimera, or into shimmering ideality. To say that social anthropological work contains, of necessity, elements of the 'artistic' is not primarily to rejoice in the uniqueness of man, and it is not to recommend that we all become poets. To celebrate the Popperian World III as the ideal where only poetic licence need be sought would be only to summon up all the easy conventions of knowledge, and to refuel the search for a real and true that poetic fiction, by definition, is both permitted and required to ignore.

Efforts to subvert the privilege of particular claims to truth are not a leap into the irrational, or an irresponsible frivolity, but an attempt to find a theoretical problematic whose powers of ventriloquism are more interesting than those to which we are accustomed. It is, after all, a more or less simply empirical discovery that the debates wherein the search for positivist reality and secure rationality is conducted are interminable, and that to escape by invoking the 'artistic' is only to 'encourage an untimely subsidence into the same old entrenchment' (Chapman 1977:94). We have been trying to examine the structure of claims to truth that have the 'power of epistemological derision' (McDonald 1978:13), and to suggest how that same symbolic power, that same metaphorical persuasion which allows of such privilege, could equally be summoned in their own belittlement and dismissal. It is already incorporated in the script that we be seen to be wandering among 'scintillating flowers' or to be having 'great fun with words' (Reynolds 1978:130). As prophesied, 'we can exploit the richness of the riddle' (McDonald 1978:27). We have tried 'to examine the space in which these arguments exist, not perhaps to secure any theoretical advance, but merely to prevent ourselves making endless journeys whose only destination is the starting point for the journey back' (Chapman 1978:43).

And all this self-reference is not merely discursive promotion; if it seems to invite accusations of idealism, then we are back in a well-rehearsed dialogue. We risk travelling again a well-trodden path between idealism and materialism, only now within the architecture of the sign where arbitrariness becomes merely unreliability, and the signified is still somewhere else, in the material world. And so on.

Malcolm Chapman

Maryon McDonald

#### REFERENCES

- Chapman, M. 1978 'Reality and Representation'. JASO IX:1 pp. 35-52.
- McDonald, M. 1978 'Language "At Home" to Educated Radicalism'.  
JASO IX:1 pp. 13-34.
- Reynolds, V. 1978 'Grasshoppers and Slugs'. JASO IX:2 pp.



## GERTRUDE

(Groupe d'Étude et de Réflexion Théoriques sur les  
Recherches universitaires et les Diplômes d'État)

### A POSTFACE TO A FEW PREFACES

One fine day, the day of their hundred and forty-third meeting, GERTRUDE decided to take stock of their situation. There was nothing brilliant about it; dejection was mounting. All the listing, scrutinising and indexing of 4,573 theses and 32,367 diplomas, all of them Africanist and produced by universities in France and abroad, had not clarified anything. A few heads - and not the emptiest or least well-shaped either - were found to have cracked definitively under it all, and their owners had taken to wandering along the corridors of the colleges and the underground with sneering grins on their faces. Others had managed to keep the balance of their minds undisturbed - but couldn't stop grinding their teeth. The question that had been scrutinized for a number of years now - 'On the sexual production of knowledge, or, The source of correct theses' - remained unanswered. Theory had nothing to say - which is intolerable when everyday practice imposes solutions of an agonizing banality. GERTRUDE gave in.

And so it was that, on that very day, a decision was taken - unanimous but for two votes (a bit too much sarcasm had unleashed a fury that could not be contained simply by the lateral chafing of the lower jaw against the upper jaw, and the two votes were exchanging blows in the corridor). It was a decision to go and consult a few eminent members of the Corps Universitaire.

One delegation set off in the direction of the Collège de France, full of hope. Barely inside the courtyard, the delegates came across a Genealogist of some repute and went straight up to him. The great man was very approachable - but all his time was taken up with a History of sexuality in six volumes. He nonetheless offered a few words of advice:

'For decades now the human sciences applied to sub-Saharan Africa bespeak a sexual division in the production of their discourse. Neither outbreaks of violence in the world, nor the structural transformations of African societies, nor even changes which, in European nations, affected both feminine participation in development and the relations between the sexes, have altered this established process - namely, the transformation of a legally married couple into a research unit of which the practices remain unchanging, whatever breaks there might be in the theoretical field in which they are circumscribed.

\*We might well express some surprise nonetheless in the face of those mutations which, from functionalism to structuralism and to marxism, far from allowing the discourse to develop in an infinite continuity, have educated it to be ready to abandon hackneyed problematics - however recent - in order to embrace new ones - themselves swiftly obsolete - to such an extent that the referential patterns articulating the final declaration, display, in contrast to the singularity of its object and

the permanence of its production practices, a burgeoning diversity. Could this mean acknowledging that, against all that ever-fluent and multiplex written parole stands a whole unsaid - perhaps an unsayable - an unformulated history of events, of relations and rules which, as the work took shape, held sway between researcher and spouse? There is nothing to prevent one taking this view - but would it not mean according a unique repressive power to the manifest discourse, a power to efface the interplay of practices without which it would all have remained in a state of parole without écriture, of thoughts without any systematicity, of a quest obstinately devoted to self-erasure through lack of materiality: just pieces of paper covered with a finite number of signs?

'In short, the prolixity of the written text should not delude us, and the task you are undertaking now tends towards a revelation of the hollowness inlaid as an intimate filigree in the overflow of Africanist discourse. Your task will involve an analysis of pages so little read that one might think them drafted only to signify nothing, to establish an irrelevance. To be brief, we are talking about prefaces, or, more precisely, about those few lines which, in one sweeping enunciation, articulate, in terms of sexual categories, the sum of operations constitutive of the final corpus.'

Encouraged by this initial success, GERTRUDE mounted a new expedition to the Collège. A few months had been enough to fix up an appointment with the Academician. With the advice of the Genealogist followed to the letter, a file of prefaces was rigorously compiled according to each and every type, and was submitted to the scholar's expertise. The conversation rarely thawed, but the illustrious mythologue willingly sketched out an analysis:

'First of all, let us consider one group of prefaces.

- P1 'My wife has assisted me by reading a mass of background literature in anthropology and history...' (Max Gluckman, Politics Law and Ritual in Tribal Society, Oxford, 1965: XXVII).
- P2 'My wife collaborated actively in all aspects of my field-work...' (V.W. Turner, Schism and Continuity in an African Society, Manchester, 1966: XVI).
- P3 'I must express my gratitude to my wife, who [...] also read the whole manuscript through with me more than once.' (I.A. Akin-Jogbin, pahomey and its Neighbours, 1708-1818, Cambridge, 1967: XI).
- P4 'My wife [...] has also materially assisted in its authorship by comment and criticism.' (Meyer Fortes, The dynamics of Clanship among the Tallensi, London, 1969: XII).

It appears from this group that the spouses have the same vocation: both are carrying out research in Africa. It appears, too, that this activity can be broken down into several moments, revealing a structure of progression: from the preparation of the field (P1), to the field itself (P2),

from the field to repeated readings of the manuscript and on to critical comments (P3, P4). At no stage do man and wife find themselves dissociated and these very variants specify a feminine attitude: in pursuing the task up to its end. From this kind of generalised undifferentiation of feminine and masculine work, should one conclude their equivalence? Certain examples would seem to suggest the hypothesis of an interchangeability of the sexes in Africanist thought and institutions:

P5 'To my wife whose book this is as much as mine.'  
(S.F. Nadel, A Black Byzantium, Oxford, 4th ed., 1961: I).

P6 'She served not only as companion in the field but also as a co-worker...' (Alvin W. Wolfe, In the Ngombe Tradition, Evanston, Ill., 1961: VI).

However, a systematic survey of the onomastic categories employed at the moment of publication reveals that it is the men who affirm themselves as sole signatories of the pieces and who easily find themselves socially acknowledged as such. The way the whole process takes place suggests a progression, as if a first confused stage - wherein spouses, collectors of different materials and undifferentiated producers of a scholarly harvest were indistinguishable - were succeeded by an era of clear-cut oppositions: the man, promoted as author of the book, lives thereafter alone in the world of culture and history, whilst the woman, of whom there remain only a few delicate memories, scattered in prefaces and dedications, returns to a state of shadowy inexistence. That original couple, self-contained and united in primitive synchrony, suffers an ineluctable disjunction since, from the preparation of the manuscript to its publication, it operates in a diachronic perspective. Now, from the original text to the printed text, not only does the message remain unchanged, but it is not unknown, too, for the wife herself, through an attentive reading of the proofs, to see to the fidelity of reproduction. Thus, it is necessary to posit a structural disequilibrium as a given from the outset, and, in spite of appearances, a secret dysharmony as the guiding force in the relations between the sexes.

From this new angle, we shall embark on another group of prefaces.

An opposition immediately presents itself: the woman, as sole commander of techniques for conserving information gathered in the field (shorthand and typing, cartography, photography...) retains use of them for herself.

P7 'My wife [ ] who typed and filed notes with love and patience...' (pierre Bettez-Gravel, Remera: A Community in Eastern Rwanda, Paris-La Haye, 1968: 10).

P8 '[ ] My wife' whose expert stenography often relieved me of the burden of recording my observations myself ...' (M.G. Marwick, Sorcery in its Social Setting, Manchester, 1965: XVIII).

P9 'Her contribution included most of the photography and much of the measuring of gardens and mapping of villages.' (V.W. Turner, op.cit.: XVI).

Pl0 'She also spent many hours typing and tabulating data...' (Richard T. Curley, Elders, Shades and women, Berkeley, Calif., 1973: IX ).

This technological inequality is further reinforced, on the psychological level, by a mass of qualities which place the feminine character in a position of superiority:

Pl1 'I wish also to thank my wife whose moral support has been expressed immeasurably in patience and forbearance.' (W.A. Shack, The Gurage, Oxford, 1966: XIII ).

Pl2 'Barbara, an unfailing source of patience, cheerfulness and support...' (F.P. Bowser, The African Slave in Colonial peru, 1524-1650, Stanford, 1974: X ).

Pl3 '... ma femme [ ... ], à laquelle je suis redevable d'un inlassable soutien dans mes difficultés quotidiennes.' (D. Zahan, Sociétés d'initiation bambara, Paris-La Haye, MCMCX: 8 ).

Holders thus of indispensable technologies, unshakeable supports of an oft demoralised partner, the women possess, too, the ability to communicate with the female members of the population under study.

Pl4 'All the information pertaining to the woman's side of the culture was gathered by her...' (M.J. Herskovits, Dahomey. An Ancient West African Kingdom, New York, 1938: XI ).

Pl5 '[ My wife ] who took many texts from women...' (M.G. Marwick, op.cit.: XVIII ).

Pl6 '... Jane, who collected the bulk of the data on the Sudanese women...' (H.B. Barclay, Buuri al Lamaab, New York, 1964: XIII ).

We recall that, according to the first group of prefaces, the spouses had an identical vocation; according to the second group, they remain faithful to it but realise it in a way that scorns any parity between the sexes. A dysfunction has been established; expressed through three codes - technological, psychological and sociological - it casts the woman as super-heroine of the research and leaves the man in a position of obligation, reduced to affirming his gratitude.

It is precisely at the point when the husband declares his thanks in the preface of the work that the initial situation is inverted: mere tributary of his wife during the actual research, he then captures the publication and turns it all to his own account. To offer thanks signifies, in a literal sense, to express gratitude, but, in a figurative sense, to

offer thanks signifies a pay-off, a dismissal. In the intimacy of the couple, the men are expressing some indebtedness; in the world of culture, they are paying off their debt. There is an equivalence between rhetorical transformation and sociological transformation, and the women would be quite wrong to accept literally any thanks offered for their research activities when the latter, devoid of any social value once completed, will be worth no more to them than to see themselves, metaphorically, paid off.'

GERTRUDE was dazzled. The exposure of the structure rendered even the more talkative members speechless, crushed by the stupefying weight of the obvious. Time passed and the interrogations were resumed. Structural awareness had given rise to exaltation, but ended up being demoralising. As luck would have it, they suddenly thought of the Theoretician, and the historical aspects of the question were feverishly pieced together. The dossier was passed on to its editor and, for greater security, to certain spinozist friends. In spite of a wealth of pre-occupations - such as upholding his thesis and the dictatorship of the proletariat - the famous philosopher wrote down, on the Paris-Amiens, his 'Reply to GERTRUDE'.

'Why these prefaces, and might they not suggest one of those moments of what we can call, with Bachelard, the 'epistemological break'? Would they not seem to mark a discontinuity between a pre-scientific practice and a theoretical, scientific practice? It is a risk to go so far as to affirm that these prefaces illustrate a decisive piece of theoretical transformation, but a risk we resolutely take. To begin with, we will place ourselves on this side of the break, in the time of its ideological prehistory. No doubt can be entertained: the dedications sublimate forms of cooperation and allow belief in a creative fusion of men and women. And there, fully at work, is the reality-masking energy of the imaginary which constitutes empirical practices into decision-making spontaneity.<sup>1</sup> Then, beyond the dedication, the men remain the sole producers, unique actors on the stage of scientific experimentation.

It is of capital import to distinguish the stages of that process which restructures, into an emergence of masculine theoretical supremacy, the indistinction of the couple during empirical practice. If, as Lenin has noted, contradiction is at work at each and every moment, then it is necessary for a non-antagonism to transform itself into an antagonism in the dominant structure of the complex whole wherein these transformations occur. Marxist theory and practice demonstrate this: everything rests in the concrete conditions. Now analysis of the productive forces and of the relations of production which take form in the initial phase reveals an immediate difference between the actors. Whilst they are interchangeable at every instant in the fieldwork investigation, one practice introduces, however, a permanent inequality: the appropriation

---

1. 'Witness particularly this preface:

"The drawings in the field, the maps and the diagrams have been made by Claudine, who also contributed her own observations and insights about Tio life. I cannot acknowledge her share in this work because love cannot calculate. In any case, it was a joint enterprise as our whole lives are." (J. Vansina, The Tio Kingdom of the Middle Congo, 1880-1892, London, 1973: Acknowledgements.)'

of the means of writing, of the typewriter, by the women.

The prefaces, like the investigations or the evidential reports, confirm the generalisation of this practice. This is not simply by chance. The moment of typing is that wherein the raw material (the information gathered so adequately called material) is transformed into a product (the typed script) by the methodical employment of means and relations of production (only the women do the typing). But production does not constitute the sum of social practice, there exist also a number of other practices, distinct in reality, theoretical practice amongst them. Now, to reduce social practice to production would mean, by fixing of the contradiction between men and women, to allow the latter a dominant position. It would, above all, be mechanically to reduce superstructure to infrastructure and to fall into 'economism'.

The true marxist tradition teaches us that every contradiction implies a real struggle and that the feminine monopolisation of typing, principal aspect of the contradiction in the practice of production, becomes secondary at the level of theoretical production. We know very well now that everything rests in the 'concrete conditions', that is in the existence of the complex whole, at the very moment when each contradiction reflects a process characterized by a dominance. Under determinate conditions - a sufficient number of typed pages - there occurs a substitution of unequal feminine and masculine roles and the man becomes dominant in turn. But the principal contradiction thus produced by displacement (transformation of roles) becomes decisive now and there occurs a condensation of the struggle at that strategic moment of the printed preface, an unstable condensation since the book constitutes a radically new form of man/woman relationship; the virilisation of theoretical practice.

We now find ourselves ready to answer the initial question 'why these prefaces?' by the exposure of their specificity. In each case, they operate as a purification, a liberation from spontaneous technical practices still subject to ideology; they radically separate the ideological elements (all the possible modes of life and work in the field) from the scientific elements (theoretical elaboration). And, in rigorously assigning limits to the domains of sexual co-production, they mark off scientific knowledge from the ideology of its past and reveal that past as ideological. It's all as clear as day.'

GERTRUDE was overcome. In one text, as dark as night, the Theoretician had repeated, with full ideological rigour, the diagnosis of the Academician. The verdict of structure found its confirmation in dialectical materialism. GERTRUDE had sought a theoretical weapon, and had been handed an instrument of hara-kiri. Happily, a few of the more combative elements took to denigrating thinkers who pretended to totalisations: they always deceived, they spoke of a by-gone world or prophesied a world to come, but the present always remained as a monstrous incongruity, corrupted by history when it wasn't spoilt by survivals. Enough of grand overviews; what was required now was detail, even sordid detail. A great fuss had, in fact, been made of a man who scrutinised texts from A to Z: a Semiologist. He, too, could be found at the Collège de France. He was charming, agreed to devote one of his seminars to the study of certain prefaces chosen for their literary merit,



and revised the results himself.

'Enfin, il convient de remercier notre épouse qui a bien voulu (jusqu'à présent) supporter les tribulations donquichottesques ainsi que les avantages et inconvénients " exotique " du métier d'anthropologue.' (Jean Copans, Stratification sociale et organisation du travail agricole dans les villages wolof mourides du Sénégal, paris, EPHE, thèse de 3<sup>e</sup> cycle, 1973, p.5.)

'Il convient de remercier notre épouse' REF. Deontological code<sup>2</sup>: a custom with the force of law demands that the woman be thanked (ousted from the publication)/ the possessive plural 'notre', indicating modesty or majesty, depending on the circumstances, reproduces an obsolete academic usage, REF. University; 'épouse' rather than 'femme', reinforced by 'notre' (SEM. Préciosité); 'qui a bien voulu (jusqu'à présent) supporter' REF. Women's Code (they are the ones who endure): this kind of endurance is the very proof of woman/ the wife's freedom metonymically suggested - she belongs to a humankind that is free - stands in antithesis to the Code that demands that, as a woman, she show forbearance (SYM. Freedom). Antithesis: accepting, refusing, placed in the balance by the 'jusqu'à présent'; 'les tribulations donquichottesque': the researcher describes himself from the outset according to literary convention, with self-evident reference both qua author and qua hero to the field of écriture/ fixed syntagm: 'avantages et inconvénients du métier' (every job has its advantages and inconveniences) / 'exotiques métier', academic connivances: reference to Bourdieu (The Sociologist's Job) and connivance with respect to the non-exotic, as shown in the use of quotation marks; every anthropologist knows that his métier is exotic only for non-anthropologists.

'Je dois maintenant rappeler l'importance qu'ont eue pour moi les nombreuses discussions menées avec L. Althusser, E. Balibar, P. Bonnafé, R. Cabanes et R. Waast ainsi et surtout qu'avec Diana Hulman, ma femme, qui a partagé avec moi aussi bien le travail sur le terrain que l'élaboration théorique.' (Pierre-Philippe Rey, Colonialisme, néo-colonialisme et transition au capitalisme, Paris, 1971, p.24.)

'Je dois' REF. Ethics, moral exigence of an autonomous subject: je. 'L. Althusser... R. Waast' SEM. Anti-hierarchical will: the millenarian convention which governs our alphabetical order lists the interlocutors in an absolutely non-significant mode/ 'et surtout qu'avec Diana Hulman', mention of first name instead of a simple initial, which distinguishes the interlocutrice from the group of interlocuteurs. 'Ma femme qui a partagé' REF. Women's Code (they are the ones who do the sharing). SYM.

- 
2. 'It has been agreed to note by SEM. those unities which constitute signifiées that will be designated each time by an approximating word (SEM. Femininity); by REF., those unities which mark cultural codes or codes of reference (REF. Code of gallantry); by SYM., unities which, in the symbolic field, take the form of antithesis (SYM. Field, Theory); by ACT., the Code of actions and behaviours.'

Field and theory: in this symbolic field, an imaginary distinction contrasts field and theory/ the fixed syntagm 'élaboration théorique' connotes the teaching of Althusser, REF. marxism in the University.

'Finally, I wish to thank my wife, to whom this book is dedicated, for her unfailing support, help and encouragement. She lived and travelled with me in this difficult region of East Africa, particularly in Turkana, where it was not thought that a European woman could easily withstand the necessarily rigorous life. She was especially important in obtaining information from the womenfolk, with whom I as a man, found serious difficulties. She herself, in addition, wrote almost the whole of The Central Nilo-Hamites which was wrongly attributed to us jointly by the editor of the series in which it appeared.' (P.H. Gulliver, The Family Herds, London, 1955; IX).

'I wish to thank my wife' REF. Code of gallantry: the woman thanked (ousted) here receives the gift of a dedication. 'She lived... difficulties': feminine Code: the woman shares. But British empiricism amasses material proofs of the wife's femininity. 'She herself... it appeared' ACT. Attempted transgression, followed by a failure. The test of the signing of a piece by the wife alone did not succeed, but it was only a half-failure.

'...last but not least is the debt I owe my wife for her graceful resignation while much of my time was being spent, mentally and physically, in the hills of Central Africa. Bringing me back from time to time to another level of reality was not the least of her contributions.' (René Lemarchand, Rwanda and Burundi, London, 1970: XI.)

REF. Economy of the gift: the preface (like any preface) is involved in an economy of exchange: the husband pays his debt by a gift superior, since it is written, to the silent effort typical of femininity. As in the case of potlatch, a gift is a test: I am giving you more than you have given me, I dominate you, I am the signatory, I have the last word. 'Bringing me back...' SYM. Antithesis Culture/Nature: too far on the side of culture, the man is called to nature by the feminine sex. REF. Feminine Code: women offer repose to the warrior.

Africanism, wife in the field, woman, constitute the signifié of the dedications. Each one poses as the unveiling of a personal, intimate relationship. Fragment of a loving, conjugal discourse? I thank her because she helps me, she helps me because she is my wife. There is a circularity here: the help refers back to the matrimonial situation which, itself, is the basic grounding for help. And might she not simply have helped me so that I would thank her? Aberrant supposition which is contradicted by a vast number of authorities, by an anteriority of patterns that make of feminine devotion a ~~not secure code~~. In announcing their thanks, the authors simply establish the reality of their conjugal relationship, but by means of narcissistic proof: I am in a position to thank her.

A narcissism often mollified, as though obliterated by good manners. It is customary to introduce one's wife when she is standing there, quite

visible, in the centre of the drawing room. The author has wanted to introduce her to his readers and she sees herself drawn out of the shadows: simple effect of social vanity. A narcissism sometimes poorly disguised by the play of connivances. The layman would smile at the naive egotism of one staging himself as some exotic Don Quixote; but the academic elite will recognise here a gravity tempered by elegant irony. Otherwise the dedication might appear a bit too heavily focussed on the ego (I owe, as for me, with me), but for the fact that scientific materialism, shared by a number of interlocutors - all professional - stands as guarantor of the relationship with the wife.'

The whole thing assumed manic proportions. All known prefaces were gone through; a publication was envisaged, but the editors back out when faced with a manuscript of almost 3000 pages. It remained in the archives. To tell the truth, the semiological initiation had not allowed an inch of progress, although it had offered some respite. Each time a manuscript of Africanist erudition appeared, we raced to the preface and forgot the rest. Back with reality, some sort of balance sheet had still to be drawn up. Structure, superstructure and infrastructure, signifier, signified, epistemology and archaeology, quite a few avenues had been explored. There was one last one left, one that was still somewhat fearsome, for so much was being said about the unconscious... Once more, contacts worked for us, and those members of GERTRUDE who could presume on a more or less distant connection with one of his disciples, went to meet the Master. Naturally, the interview was recorded in full:

'GERTRUDE: we have spent some considerable time analysing a mass of dedications, a mass of dedicaces...

The Master: Des dits cassent, words have effectivity, everyone can be heard saying it over and over again, even of the most worn-out survivors of the '20s; it's easy to say, des dits cassent, but what do they 'cassent'?

GERTRUDE: Er... well, anyway, we wanted to talk about the human sciences in Africa...

The Master: Ef-fec-tive! And there we have the whole of the new discourse of our master thinkers, of those best educated in the scientism of our time... Ef-fec-tive, as it happens, en Afrique or sans fric? The first clause that one should immediately articulate, because the exposé cannot stand the delay, is that prefaces are a riddle. Freud has warned that they should be taken as such, to the letter. Just like absurd pictures of the white-negro or of the monkey-dressed-up-to-take-first-communion, images in pre-faeces are only to be retained for their value as signifier, that is for what they allow to be spelt out of the 'pro-verb' set forth in the riddle of the pre-face.

GERTRUDE: we have already tried it on hundreds of prefaces. However, if you don't mind our saying so, we have not shifted at all; we are still behind a typewriter, derrière une machine à écrire...

The Master: machinez-vous donc et pas à écrire... la preuve, ma chine à (se)rire! And, anyway, what is le derrière d'une machine à écrire? It talks, it chatters. And what does it say - will you ask me? It has a whole history, all of its own, and reconstitutable by the most painstaking genealogist, from its-leaving-the-factory-to-the-order-slip-to-the-supplier's-bill, and it has a whole activity, whether or not she knows of its implications, whether she be typing out an invoice or an ethnological treatise, whether she agrees with it or not. And if, as is foreseeable, especially after this rhapsody, any of you should dream yourselves a type-writer, it will hardly be a matter of surprise that, when deciphering the signifier-position held by the machine in the riddle wherein the dreamer will have circumscribed his desire, we shall decipher what we can call the pre-conscious of that machine.

GERTRUDE: undoubtedly, undoubtedly, but...

The Master: But we shall still not know where it is, the derrière d'une machine. And still it is from that place that dedications effect a phantasm of l'être. L'être of the author's place. A cover, after all, and that's all. A cover, and you behind, that much is perfectly certain. But the whole enigma becomes, accordingly, clouded: la place de derrière, c'est là que ça se machine, et pas seulement à écrire.

GERTRUDE: We remain behind, even if we don't know where it is, and they, on the cover... That's what you are saying here...

The Master: yeah! Triste topique....! Leave them there where they are fully occupied. A good Africanist of that sort can be spotted at first glance: through that interior and even posterior contraction which reveals him as pregnant with the mortified foetus of his resistances.

GERTRUDE: We did not dare to think too much along those lines... (Almost in chorus) Des dix casses, casse-pipe, casse-toi...

The Master: It's coming, it's coming... and sentiment is not ruled out ...Bye! See you soon!'

For a few weeks, the proceedings of GERTRUDE's meetings remained totally hermetic. Observers were left pensive, before the bewildering dispositions of typewriters and the proliferation of the rarest and most antique editions of Almanach Vermot. Not a single visiting Africanist, whether budding thesis-slogger or recognised authority, missed out on a jolly 'Comment-vas-tu-ya-de-poêle?', but no-one ventured an 'Et-toi-le-à-matelas?'. And then it wore off.

GERTRUDE had found, by approaching leading members of the university, a few ideas to contemplate, but nothing had changed and the origin of correct theses remained as obscure as ever. One fine day, the day of the two hundred and thirty-fifth meeting, GERTRUDE at last hit upon a plan of action that was bound to work, but it was agreed unanimously - without any vote against - to keep it rigorously secret. Until now, no-one has given it away.

## REVIEW ARTICLE: THE DUTCH CONNECTION

P.E. de Josselin de Jong. Structural Anthropology in the Netherlands 1977. The Hague: Martinus Nijhoff. 399pp. Guilders 75.

In discussing the practise of anthropology in the Netherlands from the closing years of the first decade of our century, we can begin with two of the earliest and most remarkable studies that exemplify significant trends of thought.

The first is the work of van Ossenbruggen, a curious character about whom we know little except his work on Indonesian customary law and classificatory systems and a monograph on primitive thought; he died a recluse in either France or Italy in 1950. Van Ossenbruggen's paper on 'monca-pat' (1918; repub. in P.E. de Josselin de Jong, ed, Structural Anthropology in the Netherlands 1977) demonstrated the importance of the insights of Durkheim and Mauss to the understanding of Indonesian institutions and customs. Here he emphasised relations between social phenomena rather than individual institutions and customs themselves, resulting in a treatment of the social world as being composed of discrete, coherent, and totalising classificatory systems. 'Monca-pat' was related to the Javanese division of their world into four territorial groupings, corresponding to the cardinal points of the compass. Villages were arranged to form a four-fold unity around a fifth village at the intersection of the N-S and E-W axes. Effectively there was a unity between five points and four directions which was taken to represent a basic structural pattern by which the totality of existence, imaginary and real, could be incorporated. 'In other words', wrote van Ossenbruggen, 'the cruciform division of the original division of tribal territory served also as the foundation for the primitive organisation of perception' (in P.E. de Josselin de Jong 1977: 50). Classification by four and five pervaded the whole of social organisation. Thus one-fifth of the acreage of the village would be entrusted to the headman while the remaining four-fifths were cultivated by the rural population. It was the common pattern underlying the mythological relations amongst the gods and demons of the Hindu-Javanese pantheon, and further, formed the basis of the system whereby time was divided and characterised (see Pigeaud in P.E. de Josselin de Jong 1977).

It might be observed that such an analysis of ethnographic data is not uncommon, but we should remember the date of van Ossenbruggen's publication. Furthermore, his paper heralds the beginning of a tradition of fieldwork which for the first time would test empirically the adequacy of the views of the French theorists. Van Ossenbruggen's paper also contained traces of an evolutionist (or 'transformational' to borrow Locher's term) bent, inherited from Wilken, which are elegantly developed in a different direction by Rassers, whom we shall now discuss.

No papers by Rassers are presented in Structural Anthropology in the Netherlands, and although his principal work Panji, The Culture Hero, A Structural Study of Religion in Java has been translated into English, it is at present unavailable. His approach exhibited his debt both to L'Année Sociologique and to van Ossenbruggen's practical demonstration of the method, but he made an original contribution in developing an historical dimension to his analysis.

Hazeu suggested that in the guise of the ostensibly Hindu shadow play, an original Javanese ritual was prospering. By subjecting the least markedly Hindu parts of shadow play repertoire to anthropological analysis, Rassers was able to reconstitute an older corpus of myth, influential on earlier Javanese literature and dramatic production. He demonstrated that such material was structured according to a dual system of symbolic classification; however, he was unable to find a corresponding duality in

social organisation which he assumed must be responsible for the generation of such a system, and so attempted to reconstitute the conjectural form of such a social organisation from the pre-Hindu past. Thus Rassers was interested not only in elucidating the underlying principles of a classificatory system, an interest common to all the papers in Structural Anthropology in the Netherlands and central to the Leiden approach; he attempted as well to apply this structural approach to the solution of historical/evolutionary problems.

Rassers is usually credited with introducing J.P.B. de Josselin de Jong to the work of L'Année Sociologique, and with the latter's election to a chair at Leiden in 1922 we mark the beginning of the structural tradition in the Netherlands. From these early years increasing attention was given to classificatory systems as they reveal the perceptual organisation that a people impose to order its world, and the field of study was widened to take into consideration Indonesian societies outside Java. The type of structural pattern van Ossenbruggen was able to discern in Java was studied as one possible variant of many which appeared to have characterised Indonesian societies in the past and to have left important traces in the present (see Onwlee and Jansen in P.E. de Josselin de Jong 1977). A recurrent similarity of these Indonesian societies appeared to be a kinship structure based on a moiety system constituted by two pairs of mutually crossing moieties, resulting in double descent; the same classificatory principle, furthermore, was thought to structure the religious order - a classically Durkheimian position which invested the social organisation with determinate power. Over and above this, it was also supposed that the logic underlying symbolic classificatory systems was governed by certain unconscious, structural principles which found their most elaborate expression in kinship relations. On this set of presuppositions van Wouden based his scholarly researches into Types of Social Structure in Eastern Indonesia (1935). By suggesting that any model is capable of implying diverse possibilities, other than those realised in empirical reality, van Wouden was able to isolate and account for the relations between a system of asymmetric, prescriptive alliance and matrilineal and patrilineal forms of social organisation. During the same year and under the same influences G.J. Held conducted a similar analysis of the social organisation underlying the Mahābhārata confirming some of van Wouden's theses.

Many similarities are evident between the work of the Leiden scholars and the later writings of Lévi-Strauss (although Lévi-Strauss makes no mention of their work in Les structures élémentaires de la parenté 1949). It may, indeed, be argued that the early exponents of Dutch anthropology, viz. van Ossenbruggen, Rassers, and van Wouden, had anticipated the course that French thought was to take. Leiden, of course, was being heavily influenced by the work of L'Année sociologique at the same time as Lévi-Strauss was himself a student of Mauss. Van Ossenbruggen's work on territorial classification in Java was directly influenced by Mauss's essay on primitive classification, and it in turn stimulated new studies in that idiom by Dutch scholars. It is not surprising, therefore, that the first appreciation and commentary on Lévi-Strauss' Les Structures élémentaires de la parenté was written by J.P.B. de Josselin de Jong (1952; repub. in P.E. de Josselin de Jong 1977). Apart from some critical comments on matters of detail regarding Lévi-Strauss' rendering of the Australian classificatory systems, he applauds the analysis.

These last criticisms of de Josselin de Jong to Lévi-Strauss tell us something else about the Leiden tradition: it was concerned with structuralism only in so far as the theory was of use in interpreting and explaining social reality. P.E. de Josselin de Jong has written that, whereas for Lévi-Strauss structuralism was a means of revealing the fundamental mechanisms of the human mind, the Leiden anthropologists sought only the description of an 'ordered-structured-universe'. The theoretical principles involved had been largely ignored, as had their methodological presuppositions. At any rate modern



Dutch writers show a greater interest in theoretical matters; see, for example, van Baal's Symbols for Communication, 1971, and Reciprocity and the Position of Women, 1975; also de Josselin de Jong and papers by J. Pouwer. P.E. de Josselin de Jong described Leiden anthropology thus:

The aim has always been a harmonious combination of empirical work in the field and thorough theoretical preparation for, and analysis of, this work (P.E. de Josselin de Jong 1960: 16).

Like the British tradition, Dutch anthropology went through a period of evolutionist thought, and it was only under the guidance of J.P.B. de Josselin de Jong that sociology in the contemporary sense was established. In discussing his enormous influence, van Baal notes that without being a prolific writer (much of his own fieldwork remains unpublished) he was able to raise the level of anthropological discussion by moving it away from the accumulation of ethnographic snippets towards a coherent, empirical enquiry into the structural orders of societies (van Baal 1965). De Josselin de Jong made an analogy between society and language which, together with the Durkheimian influence, led him to advocate a holistic attempt to uncover the fundamental structural configurations underlying a society. He writes, for example:

Man is no more conscious of the system as such, than he is of the grammatical construction of his language. But he applies the system nevertheless and is guided by it in all his activities, in a way similar to that in which he uses the system of his language and at the same time is controlled by it in his speech. (J.P.B. de Josselin de Jong in P.E. de Josselin de Jong 1977: 174).

It is not surprising that early Dutch anthropologists tended to concentrate their studies on Indonesia. They were mostly colonial administrators, missionaries, linguists, and jurists, who could indulge their interest in exotic cultures only during their spare time. Unlike so many others, however, their professions did not impair their scholarship and their pastime tended to improve colonial rule. With the end of the Second World War, Indonesia was soon to gain her independence; relations between the Netherlands and her former colony had seriously deteriorated, making it extremely difficult to continue ethnographic work there. This resulted in a re-orientation of studies which primarily focused attention on Irian Jaya. Under van Baal's governorship, anthropological research was enthusiastically encouraged and apart from his own scholarly work on the structure of Marind-Anim religion, numerous other monographs appeared dealing with many of the main ethnic groups (see Held 1957; Serpenti 1965 etc.). Some of the researchers addressed their work to particular problems (such as Pouwer and Schoarl) but there remained a substantial corpus of work on symbolic classification and some on religion (not only related to Irian Jaya; see van Zantwijk 1967 on the Tarascans, and van der Leeden 1975 on Australian aboriginal mythology).

A third period characterised by a further re-orientation of fieldwork was necessitated in the 1960s by the Indonesian colonisation of Irian Jaya and the expulsion of the Dutch. From that time Dutch anthropologists have widened their interests to include South and Middle America, Africa, and the Arctic lands, with an increasing number of undergraduate students carrying out fieldwork in rural communities in Europe. With the diversification of the field of research has come exploration into different methodological approaches, but despite P.E. de Josselin de Jong's opinion that the 'Leiden trend' has lost much of its peculiar character it is still possible to discern a distinct tradition in the Netherlands.

It is astonishing that, as far as I am aware, all the written histories of anthropological thought published in the English language have failed to note even the existence of Dutch anthropology. British and American students owe a great debt to the work of Rodney Needham, not only for introducing Leiden anthropology to an English-speaking audience, but for the translations



he has given us, beginning with Durkheim and Mauss on Primitive Classification and including such Dutch authors as Pouwer, Pott and van Wouden. This new collection of essays edited by de Josselin de Jong makes a stimulating addition to the growing availability in English of Dutch works already commissioned by Van Gorcum and the Koninklijk Instituut voor Taal-Land-en Volkenkunde, and it attests to the esteem given them by their British colleagues.

Anthony Shelton

REFERENCES

- Baal, J. van Dema 1966 Description and Analysis of Marind-Anim Culture (South New Guinea). The Hague: M. Nijhoff.
- 1971 Symbols for Communication. An introduction to the Anthropological Study of Religion. Assen: Van Gorcum.
- 1975 Reciprocity and the Position of Women. Assen: Van Gorcum.
- Durkheim, E. and Mauss, M. 1963 Primitive Classification. Trans. and ed. by R. Needham. London: Cohen and West.
- Held, G.J. 1935 The Mahabharata. An Ethnological Study. RKP.
- 1957 The Papuans of Waropen. The Hague: Martinus Nijhoff.
- de Josselin de Jong, J.P.B. 1977 The Malay Archipelago as a Field of Ethnological Study, in P.E. de Josselin de Jong 1977.
- 1977 Lévi-Strauss's Theory of Kinship and Marriage, in P.E. de Josselin de Jong 1977.
- de Josselin de Jong, P.E. 1960 Cultural Anthropology in the Netherlands, in Higher Education and Research in the Netherlands. 4:3-16.
- (ed) 1977 Structural Anthropology in the Netherlands. The Hague: Martinus Nijhoff.
- Kloos, P. and Claessen, J.M. 1975 Current Anthropology in the Netherlands Netherlands Anthropological and Sociological Society.
- Leeden, A.C. van der 1975 Thundering Gecko and Emu: Mythological Structuring of Nunggubuju Patri-Moieties, in L.R. Hyett (ed.), Australian Aboriginal Mythology. Sydney: Australian Institute of Aboriginal Studies.

- Lévi-Strauss, C. 1949 Les structures élémentaires de la parenté Paris: P.U.F.
- 1974 Structuralism and Ecology, Social Science Information, 12:1.
- Needham, R. 1960 Review: W.H. Rassers: Panji, The Culture Hero. American Anthropologist 62:174-6.
- 1971 Introductory essay in Rethinking Kinship and Marriage, A.S.A. Monographs 11, London: Tavistock.
- Onvlee, L. 1977 The Construction of the Mangili Dam. Notes on the Social Organisation of Eastern Sumba, in P.E. de Josselin de Jong, 1977.
- Ossenbruggen, F.D.E.  
var. 1977 Java's monca-pat: Origins of a Primitive  
(1916) Classification System, in P.E. de Josselin de Jong, 1977.
- Pott, P.H. 1966 Yoga and Yantra: Their Interrelation and their Significance for Indian Archaeology  
R. Needham trans. The Hague: Martinus Nijhoff.
- Pouwer, J. 1966 The Structural and Functional Approach in Cultural Anthropology. Theoretical Reflections with Reference to Research in Western New Guinea. B.T.L.V. 122:129-44.
- 1966 Referential and Inreferential Reality. A Rejoinder. B.T.L.V. 122:155-7.
- Rassers, W.H. 1959 Panji, The Culture Hero. A Structural Study of Religion in Java. The Hague: Martinus Nijhoff.
- Serpenti, L.M. 1965 Cultivators in the Swamps. Social Structure and Horticulture in a New Guinea Society. Assen: Van Gorcum.
- Wouden, F.A.E. van 1968 Types of Social Structure in Eastern Indonesia. R. Needham trans. The Hague: Martinus Nijhoff.
- Zantwijk, R.A.M.  
van 1967 Servants of the Saints. The Social and Cultural Identity of a Tarascan Community in Mexico. Assen: Van Gorcum.

## BOOK REVIEWS

Barbara Nimri Aziz Tibetan Frontier Families, Reflections of three Generations from D'ing-ri. 1978, pl. 20, pp xiv, 292. New Delhi: Vikas

Vikas are to be thanked for publishing this long-awaited study to accompany their recent reprinting of Victorian classics of Himalayan and Tibetan exploration. Anthropologists, in working with culturally Tibetan peoples on the southern slopes of the Himalaya, have been long aware of the absence of more than anecdotal knowledge of the circumstances of these same peoples to the north of the Tibetan border. This study of the people of D'ing-ri, a district of some 1000 sq.kms. and 12,000 people, north-west from Kumba on the main trade-route from Nepal to Lhasa, goes some of the way towards filling this ethnographic gap.

Barbara Aziz, who completed her Ph.d. at SOAS under Professor S von Furer-Haimendorf, characterises herself here as a biographer of rural heroes, and the work as an historical portrait, over three generations, of the people of D'ing-ri. The research was carried out in 1970, 1971 and 1975 with people from D'ing-ri now resident in Nepal, and incorporates her earlier published work on Descent and Residence in this community ('Some Notions of Descent and Residence in Tibetan Society', in Contributions to the Anthropology of Nepal, G. von Furer Haimendorf, U.K., Phillips and Aris, 1974). To construct an ethnographic picture from a refugee population is a difficult undertaking, and Barbara Aziz is to be congratulated on her perspicacity in attempting such work. In so doing her focus is, perhaps of necessity, not the single time and place of a village study, but a general consideration of the entire area over a lengthy period. In such a work there are evident limitations of method, and the inclusion of forty-three case studies as an integral part of the text will allow the reader to judge for himself the nature of the information on which the analysis is based.

The style of the work is unconventional not only in ethnographical format, but also in combining the impressions of individuals, and evaluative, dramatic statements, with social analysis; for example, it is not clear what is meant by 'the spontaneous foundation of a heterogeneous society' (p.50). There are simple errors which careful editing would have eliminated; for example, the 'great-little tradition' was proposed by Redefield, not Tambiah (p.203), the plates are un-numbered, and the correct Tibetan transliteration of ge-tsul is dge tshul, not sge-tshul (p.272). It is hoped that such faults will not deter serious readers, as the book is both a unique source of information on this region, and theoretically stimulating in containing an analysis of a practically unstudied Tibetan institution, the village community of lay-priests who in D'ing-ri are known as the ser-ky'im.

The first chapter gives an ecological account of the region, which together with the nine maps, and the appendices of transliterations of place names, is extremely useful for any student of the area. The second and tenth chapters provide biographical histories of Tibetan religious figures who visited the region; the third puts forward a general framework of social classification, both for D'ing-ri and Tibet as a whole. The remaining seven chapters give an account of economic exchanges, descent and marriage as they centre around the household, and of non-kinship forms of social organisation, namely, friendship

societies, lay-priests, and relations with monasteries and other arms of the administration. Two interesting phenomena are described for specialists in kinship and marriage. The first is that lineages occur only for priests, nobility and outcasts, not for the majority of the people, the commoners whose link to particular land results in the grouping of rules of marriage and inheritance around residence in a household. The second is the significance both of hypogamy and hypergamy for these same household units, here looked at respectively as tactics for obtaining prestige and labour viewed from the system as a whole, such an assymetric form of marriage cannot fail to interest students of South Asian kinship organisation. The general framework is a four-fold classification into priests (t: sngags-pa), nobles (t: sger-pa), commoners (t: mi-ser), and outcastes (t: ya-ba). This is a system similar to the Hindu 'jati' (p. 52, jajmani ?), or ideological system of varna, but without the pronounced concept of defilement; these are points that deserve to be developed further than they are in this work. We may accept that in D'ring-ri there is a class of hereditary priests called ngag-pa, but as a general framework for Tibet is being proposed, we would like a fuller consideration of this status than is provided by the notion of dung-gyu as a spiritual quality possessed by the priests alone. A Tibetologist would be sure to point out that (t:) sngags is a literal equivalent of the Sanskrit mantra, that (t:) gdung is the honorific for 'lineage' or 'descent', that (t:) rgyud 'connection', and that (t:) brgyud, means 'connection' with the specific connotation of 'descent': as an honorific for descent, the term would be as applicable to nobles as to priests. The quality that the nobles supposedly possess through inheritance, the ku-gyu-pa, could perhaps also be clarified by considering what the term generally means, one of the translations of (t:) sku being 'body'.

Although it is later pointed out that it has the general meaning of 'tax-paying householder', dr'ong-ba (t: grong-pa) (the class of commoners) is subdivided in such a way that this term is glossed as 'agriculturalist'; this is misleading, as others besides dr'ong ba cultivate the soil. There appears to be little utility in the analytical divisions, as is further instanced by the fact that lay-priests are both commoners (p. 10), and together with lower groups of commoners opposed to upper commoners, nobles and priests (p. 161). That this should occur with an indigenous social classification that is used as a framework for an empirical model of social groups is understandable. It raises the question, however, of whether such a framework can be used, on its own, for social analysis in a society which is not static but allows a degree of personal mobility, and has undergone institutional change. Individual and institutional changes in status, in short history, are problematic for any ethnography that takes as its main model a static framework from the ideology of the people themselves. It is, however, only through the data and analysis given by DBZ, both of marriages between assymetric statuses, and of a possible evolutionary relationship between monasteries and communities of lay priests, that allows us to single out the theoretical question, to which it is hoped more attention can be given in the future. Despite these minor reservations, this book is an indispensable source of information for sociologically oriented students of Tibetan society.

Graham Clarke

Population Problems in the Victorian Age. Debates on the Issue from 19th Century Critical Journals. Vol. I Theory; Vol. II Statistics. edited with introductions by J.J. Spengler. Farnborough, Hants: Gregg International Publishers, 1973 unpag. £8.00

One of the principal mysteries of the history of population analysis, at least in retrospect, is the separation of debates about population increases from the development of the apparatus for measuring these changes. In the nineteenth century scarcely a decade passed without an attempt to reformulate or defend Malthus; however, the questions necessary to understanding fertility and the statistical mechanics of increase did not appear until the present century. It is only with the formal elaboration of the fertility concept that what is now called 'demography' came into being; in the last century, despite collection of some marriage and birth data, and the development of a sophisticated understanding of age structure, the mathematics of population remained the mathematics of mortality.

To raise this question of separation is not to read current issues and formulations back onto a prior period, for rather minimal calculations of fertility and vitality did appear in the nineteenth century. What is of interest is how certain questions led to certain sorts of apparatus with differing possibilities of development. From the course of history we know that fertility analysis awaited Darwin's reformulation of Malthus, which gave rise, among other things, to mathematical biology. Why should the formal apparatus for describing increases appear in this context, and not in a long period of direct and intense interest in human population increases? Carefully examined, the contrast of the various developments may tell us something of the nature of blockages in formal analysis. The interest is not in the questions that were not asked, but concerns the closure effected by those that were. It hardly needs repeating that anthropology has a considerable interest at present in understanding what stops formal methods from maturing.

Although the two volume organization of these nineteenth century reprints suggests the separation of issues and available statistics, it does not in fact reflect it. This is because the 'Statistics' volume, while fairly representative of mathematical papers in journals of the time, is, by this limitation, not representative of practice elsewhere (e.g., in actuarial societies, or the Registrar-General's Office). Hence both volumes are concerned with topical issues and not the development of the formal apparatus.

Nonetheless, taking as an example three articles which cluster around mid-century, we can get an idea of the diffuse state of analysis. In the 'Statistics' volume there is an article published in the Quarterly Review (1845) on the Census of 1841. The paper is less about the census than it is a paean to the advantages of calculating percentages of different subgroups (numbers of soldiers dying of tropical diseases, changes in the number of persons per dwelling, etc.) in order to assess social vitality or well-being; the techniques employed show little advance over Graunt's Observations of 150 years before. Another article in the volume, taken from Blackwoods (1851) demonstrates another common practice of the time, the comparison of census tabulations with other statistics. In this case, data on the Irish emigration during the famine are combined with trade figures in order to show that free trade induces population declines. Elsewhere in the volume, there are other ad hoc comparisons, for example, of the relation of population density and immorality to changes in the rate of increase. Reprinted in the first volume is Herbert Spencer's curious paper published in the Westminster Review (1852), in which he reasons that population

increases serve the evolutionary purpose of stimulating the development of the human nervous system; greater intelligence is needed to cope with the increased problem of subsistence, etc., in more populous societies; and greater intelligence, in turn, leads to better regulation of numbers.

These papers show little technical awareness of the actual conduct of population analysis at the time. They take unrelated products of analysis and relate them by arithmetic reasoning to other external issues, principally that of social 'vitality'. The interest in ~~issues~~ <sup>issues</sup> takes the form of 'What limits production (socially or materially)?', which is not at all the same question as that of fertility, viz., 'What is the nature (the mathematical regularity) of productivity?'. The first uses ~~numerical~~ relations in an illustrative way, whereas the second argument has a partially mathematical structure. Under the former conditions the possibility of constructing apparatus or partly mathematical systems does not arise.

This difference between fertility and vitality, while an important marker of the period, is not by any means sufficient to account for the absence of formal developments; the co-existence in this century of fertility analysis and popular movements (ecology; birth control) at least suggests otherwise. Indeed, it is the capacity of more topical questions regarding vital forces to cut themselves off from relations of ((theoretical)) production - their capacity to distract-which suggests that the problem can never be effectively addressed without examination of the character and effects of apparatus.

The tendency to discuss topical issues to the exclusion of contemporaneous methods of formal analysis, and vice versa, characterizes not only the history of population analysis, but the history of methods in social sciences in general. It is disappointing to see this pattern repeated in the present volumes. What is the point of reprinting or unpacking this history unless it is to identify the historical limits of population analysis, and so to improve or change it?

There is however a good amount of sustenance to be gained from these reprints, both on the relation of population topics to political economy, and on administrative aspects of the early registrations. The papers will undoubtedly be very welcome to those without access to the original journals (in addition to those already cited, there are reprints from Fraser's and the Edinburgh Review). The photographic quality of the reprints is excellent, although it is regrettable that while each paper retains its original pagination, the two books as a whole have none. This, together with the failure to cite authors (these have to be ferreted out of Spengler's introduction), makes the table of contents of somewhat limited use. Those who are familiar with Spengler's other works on early population and economic analysis are likely to be disappointed by his very brief introductions. In particular, no rationale is given for the selection of articles, and there is only cursory treatment of their inter-relation.

Phil Kreager



Herbert G. Gutman: Work, Culture and Society in Industrializing America.  
Oxford: Basil Blackwell. 1977 x, 343pp. £10.50.

Working class history in America has always been a problematic subject for American historians. For one thing, it is a peculiarly American myth that the working class per se--a group of men and women linked together by the common denominator of their wage labour, fecund with their own cultural traditions and identity--does not exist. The class conflicts which have marked every nation in Europe are absent in America; divisions perhaps exist between 'white collar' and 'blue collar' workers, but classes as such seem un-American - so the argument runs. Instead American culture has given itself a myth of consensus, a myth of itself as the first conflict-free society, whose material abundance and sense of mission must lead it to be better, richer, more ideal than any before it. The underlying consensus of value can thus absorb any lingering social ills in a vision of inevitable progress; present need finds solace in future surfeit.

This myth of consensus has particularly shortchanged the historiography of early industrialization in America. The 'rags to riches' myth preached by such writers as Horatio Alger and Samuel Smiles in fact obsessed historians as well, reinforcing thrift, hard work, simple living and planning for the future as the essential determinants of 'American character'. Because these were viewed in purely moral terms, and because material gain was seen as the reward of moral virtue, historians of industrial America tended to focus on the upwardly mobile and the individually successful, taking them as symbols of the national 'consensus'.

This is the historiographical myth which H.G. Gutman has sought to criticize in the series of essays recently published as Work, Culture and Society in Industrializing America. Basing himself very explicitly on the work of E. P. Thompson in Britain, Gutman tries to elucidate the concepts necessary to a history of the American working class. Disavowing Tonnies' classic distinction between Gemeinschaft and Gesellschaft as too simplistic, he draws instead upon the work of the anthropologists Eric Wolf and Sidney W. Mintz, as well as on the sociologist Zygmunt Bauman. Gutman's title shows his anthropological concern: for he uses the distinction between 'culture' and 'society' to account for both the changes and the continuities of the American working class as it was transformed by industrial society. In contrast to both the functionalist diminishment of 'culture' vis-a-vis the organic model of 'society' and the semantic interpretation of 'society' itself as merely a domain of cultural meanings. Gutman treats these as separate and interrelated tools of historical analysis. 'Culture' is the broad set of concepts and ideas--the modes of understanding and action--which a human group has available to it in the context of its past experience; 'society', the more limited term, is the set of real historical contingencies which the culture must act upon. Thus he avoids both the objectifications of functionalism and the idealizing of semantic anthropology; in contrast to the ahistorical tendencies of each, he can give an account of both continuity and crisis. As Gutman himself says,

An analytic model that distinguishes between culture and society reveals that even in periods of radical economic and social change powerful culture continuities and adaptations continued to shape the historical behaviour of diverse working-class populations (WCS, 18).

By paying attention to both the radical changes and powerful stabilities in American working class life, Gutman shows us a richer field of interpretation than we have previously seen. His concentration on local history, as opposed to national sources, bears special fruit since the groups studied were never powerful nor even very visible nationally. Working class kinship patterns, mobility rates, cultural mores all raise issues that must necessarily be studied at the local level. Gutman has devoted considerable time to tracing the history of the textile town of Paterson, New Jersey, a town rich in what William Carlos Williams called 'the anarchy of poverty.' In one essay Gutman demonstrates that in the post-Civil War expansion, the town's new industrialists were not well-integrated into the older community whose economy they now dominated. Labour disputes frequently saw Paterson's non-industrial elites--local government officials, newspaper editors, small tradesmen, professionals--side with workers as often as factory owners. The new economic power of the manufacturers was not immediately transformed into social status, and Paterson's class lines showed anything but the simplicities that vulgar Marxism might expect. Public police forces rarely gave whole-hearted support to the owners - who thus had to hire their own police power and strikers were not punished for exercising 'peaceful coercion' in persuading scabs not to work. The link between social status and economic power did become closer as the old pre-industrial middle class was eroded by time, but Gutman's point is nevertheless well-taken: there was a time-lag between the new society and the old, and the old culture was slow to relinquish its values to the needs of a factory society.

Another of Gutman's contributions is to avoid too narrow a focus on trade unions, a concentration which has marred most American labour history. It is here that Gutman's debt to the new school of British historians is most marked. In a manner reminiscent of Thompson's The Making of the English Working Class, he discloses in America the pre-industrial work habits which we have already encountered in England: the changing pace of work in the course of a normal week, with long weekends of drinking, gaming, and debauchery ending in the traditional 'Blue Monday' of late arrival on the job. In an impressionistic essay which suggests rather than exhausts the possibilities for a new working class history, he points out instances of gang culture, food riots, Luddism, and violence which mitigate against the usual picture of American 'consensus'. He demonstrates, as Thompson has done, that the working class resisted both the techniques of the new work and the regular hours of the factory - a problem compounded in America, as it was not in Europe, by the regular renewal of the immigrant work force. Each new wave brought with it diverse cultural backgrounds, whether industrial or pre-industrial, which had to be fitted to the Procrustean rule of factory efficiency. Thus Gutman quotes a chillingly coercive textbook with which the International Harvester Corporation taught its Polish labourers the English language:

I hear the whistle. I must hurry.  
I hear the five minute whistle.  
It is time to go into the shop.  
I take my check from the gate board and hang it  
on the department board.  
I change my clothes and get ready to work.  
The starting whistle blows.  
I eat my lunch.  
It is forbidden to eat until then.

The whistle blows at five minutes of starting time.  
I get ready to go to work.  
I work until the whistle blows to quit.  
I leave my place nice and clean.  
I put all my clothes in the locker.  
I must go home. (CS, 6).

The march from rags to riches was evidently a well-regimented one. Gutman gives us much other material as well. There is an extended study of the ways in which pre-millennial Protestantism was used to convert Christianity into a revolutionary labour doctrine, and a long essay about the work of an early black trade unionist, Richard L. Davis, who tried to bridge racial barriers in extending the United Mine Workers to black miners. These and other, more traditional studies deepen our understanding not only of the communities he portrays, but also of the methodologies relevant to rediscovering a side of American culture previously obscured.

The book is not without its problems. The essays are uneven in quality, betraying their earlier form as published monographs. It lacks a bibliography, and little attempt has been made to tie the essays together into a coherent whole: the introduction is too brief, there is no conclusion at all, and each chapter is made to stand very much on its own. Gutman's writing style is no more than clear, and its lack of polish gives it somewhat the quality of a scrap-book well-pasted with clippings.

These are not, however, damning weaknesses. Work, Culture and Society is an important contribution to our understanding of nineteenth-century working-class culture and the social structure within which it existed. In attacking the consensus version of American history, it opens the way for a more 'anthropological' examination of the American past. The ways in which kinship patterns encouraged and reinforced the creation of the Tammany Hall boss system; the religious underpinnings of interracial working class solidarity in the trade union movement; the ethic of violence as the concomitant of a culture in social upheaval—all of these deserve fuller and more extended exploration. Not only does Gutman's work remind us that 'the traditional imperial boundaries' of academic study have prevented the broad synthesis necessary to cultural history; it also provides evidence of the richness that can result when such boundaries are traversed. Gutman's reassimilation of immigrant, racial, urban, and labour history for the portrayal of working class experience has been needed in American history for a long time. Avoiding class concepts borrowed too mechanistically from the European experience, Gutman still manages to locate the notion of 'class' within mainstream American history. At the same time, he reminds us of the community attachments which Clifford Geertz has labelled primordial: 'the 'assumed' givens...of social existence: immediate contiguity and kin connections mainly, but beyond them, the givenness that stems from being born into a particular religious community, speaking a particular language and following particular social patterns' (CS, 43).

William Cronon

BOOKS RECEIVED

- Richard N. T-W-Fiennes. The Environment of Man. London: Croom Helm.  
1978. 154 pp.
- Stephen Gudeman. The Demise of a Rural Economy. London: RKP.  
1978. vii, 176 pp. £6.95 cloth edition,  
£3.75 paperback.
- Johan Adrian Jacobsen. Alaskan Voyage 1881-1883. Trans. E. Gunther.  
Chicago: The University of Chicago Press. 1977.  
xii, 266 pp. \$17.50.
- Ivan Karp. Fields of Change Among the Iteso of Kenya.  
London: RKP. 1978. x, 186 pp. £6.95.
- William H. Kelly. Cocopa Ethnography. Tucson: University of  
Arizona Press. 1977. vii, 150 pp. \$7.95.
- Georg Knepler. Geschichte als Weg zum Musikverständnis.  
Leipzig: Reclam. 1977. 658 pp.